JUN -3 1949

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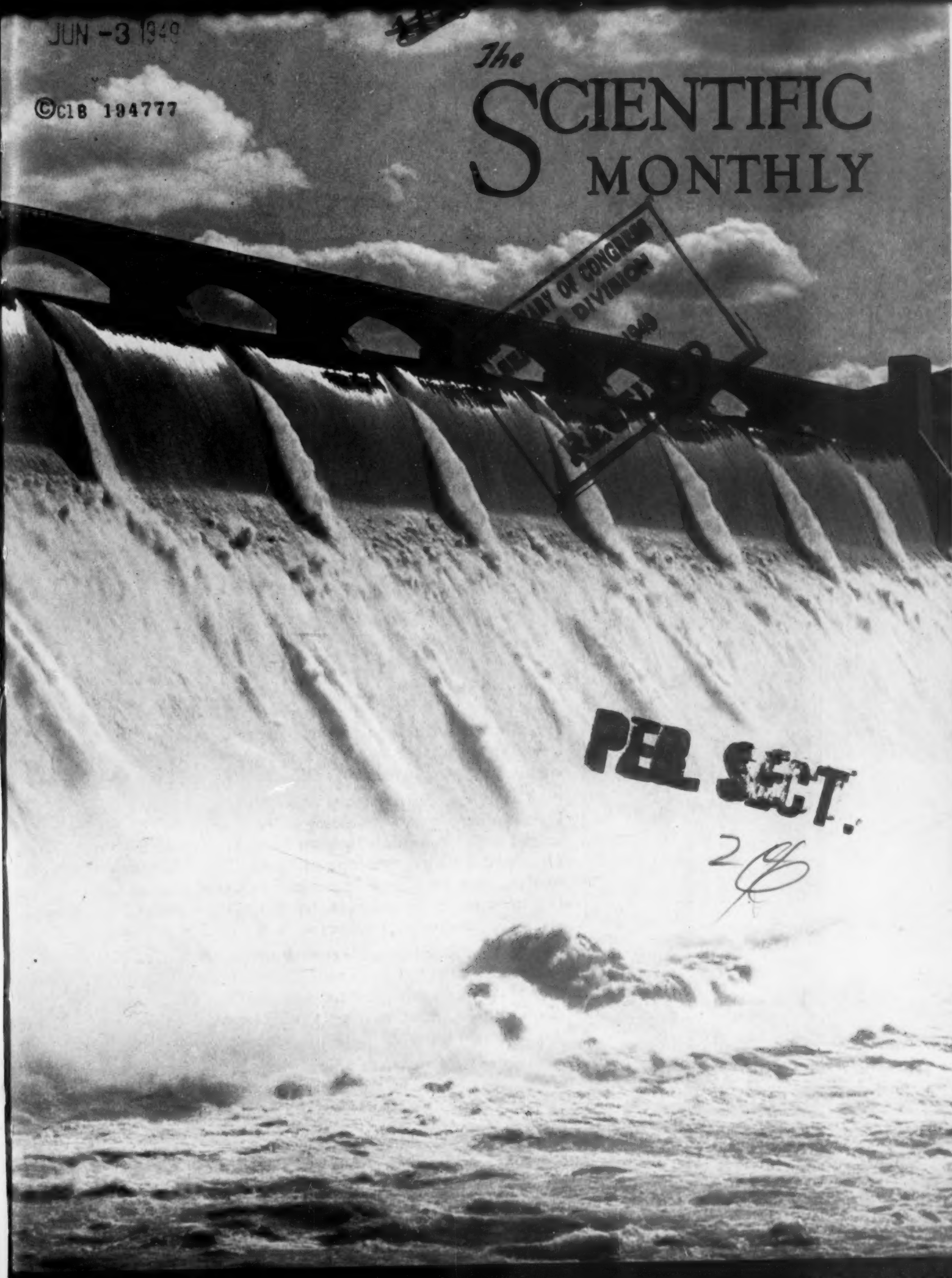
The SCIENTIFIC MONTHLY



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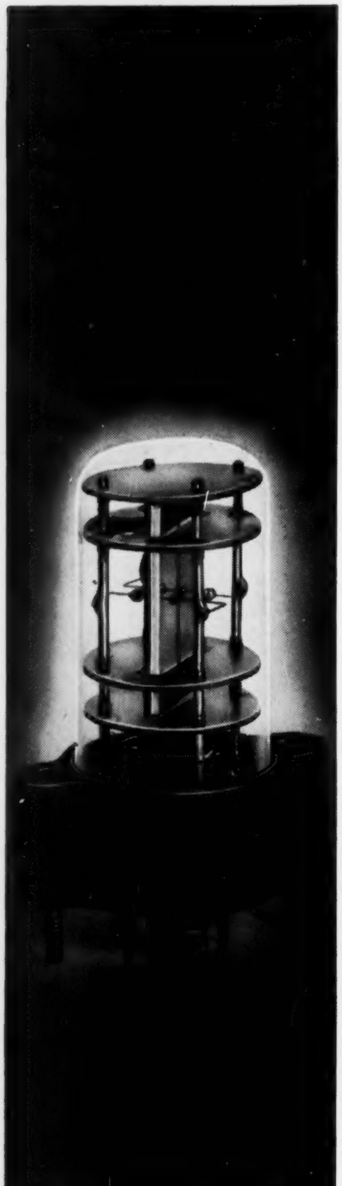
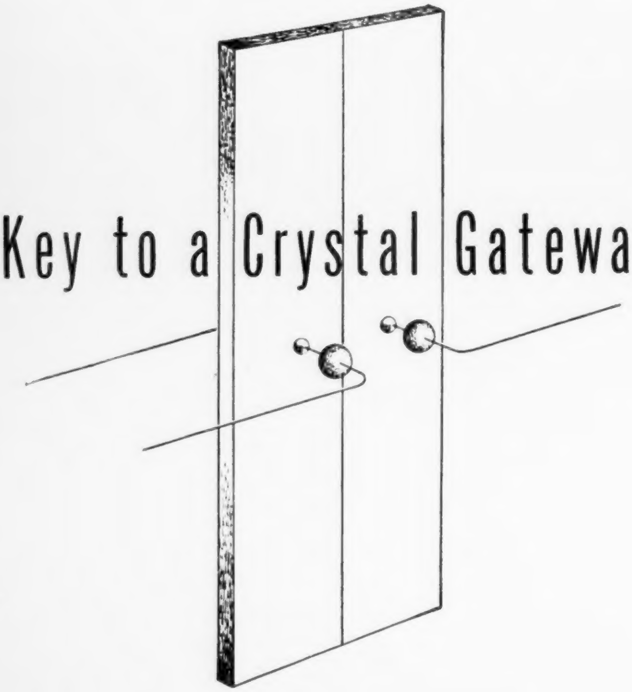
VOL. LXVIII NO. 6

JUNE 1949



Above is a coaxial circuit crystal in its glass enclosure. At right the crystal is shown $3\frac{1}{2}$ times actual size, with connecting wires soldered in position. Weights on wire reflect energy back into crystal, so cut losses.

Key to a Crystal Gateway



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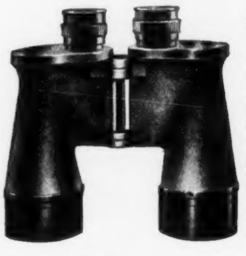

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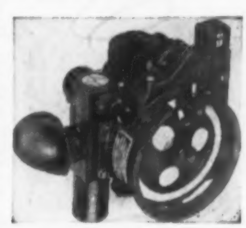

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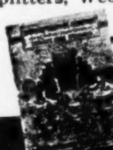

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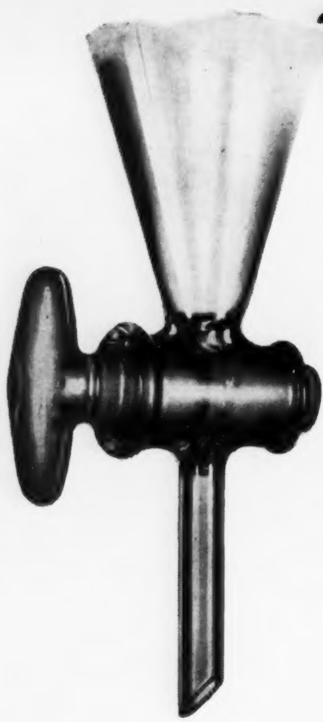

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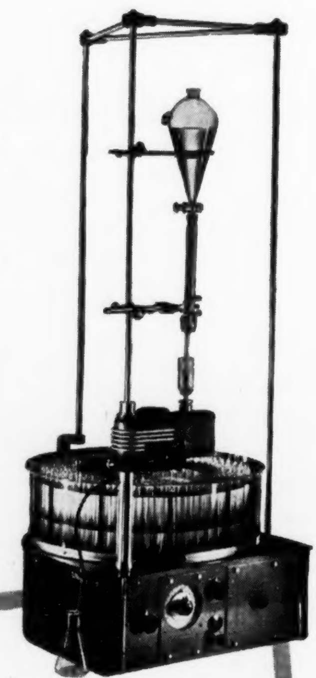

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THE SCIENTIFIC MONTHLY

VOL. LXVIII — JUNE 1949 — NO. 6

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Science and Technology

(From the Month's News Releases)

Push-Button Living

With the Robot Door Operator you can now unlock and open your garage door electronically. Touch a button on the instrument panel of your car as you enter the driveway, and yard and garage lights go on simultaneously. A second button controls the closing and locking of the garage door, besides turning off the lights.

Another robot device, the tele-magnet, answers your telephone when you are out, with the report that you are not at home, and that a wire recorder will take a message that you will play back when you return. The device is about the size of a small table phonograph, can record up to 60 minutes of messages, and will probably sell for about $200. Messages can be erased and the recorder reset.

Down on the farm, even feeding the chickens has been reduced to a push-button operation. By means of an electrically operated conveyor, time-clock controlled, the farmer can supply enough chicken feed to provide for his flock while he is away enjoying a weekend holiday.

New Publications

From the National Bureau of Standards: *Steel Reinforcing Bars,* 11 pp., 10 cents; *Mineral Wool Insulation for Low Temperatures,* 26 pp., 19 cents; *Microbiological Deterioration of Organic Materials—Its Prevention and Methods of Test,* 39 pp., 25 cents; *Douglas Fir Plywood,* 25 pp., 10 cents; *Copper Naphthenate Wood-Preservative,* 9 pp., 10 cents; *Temperatures in a Test Bungalow with Some Radiant and Jacketed Space Heaters,* 44 pp., 25 cents; *Fire Resistance of Structural Clay Partitions,* 19 pp., 15 cents.

NoDrip

An improved cork-filled tape is now available to stop dripping on cold water pipes. NoDrip tape, wound spirally around straight pipes, valves, joints, and tees, will form a tight-fitting, sealed jacket. A standard package ($1.69) contains enough tape to cover about 10 feet of 1/2-inch pipe.

Nontoxic, Fire-retardant Paint

A paint that can withstand a blow-torch flame for 30 minutes on one spot without burning is being manufactured under the name of "Flame-Seal." The Flame-Seal coating, when subjected to fire, forms a white crust that creates a hard, protective wall at least eight times the thickness of the original coating, according to the manufacturers. The new paint is described as having nontoxic fumes in application, as being moisture- and termite-proof, and not subject to chipping, peeling, or cracking.

Perma-Broom

A lightweight broom, with "permanent" bright-colored bristles, is said to be more durable and more effective than the standard straw broom. It holds its shape and can be washed with ease after every using.

Books Abroad

There are now two methods by which foreign institutions may obtain American books. UNESCO Book Coupons are available in some countries for purchase of U. S. books through the American Booksellers Association, New York City. The CARE Book Program was launched in April 1949 to provide new books on university and research levels to institutions in any country served by CARE. The bibliography is supervised by the Librarian of Congress.

Plastic Wallpaper

One of the devices for combating radioactive pollution of laboratories and factories is a plastic wall coating that may be stripped off to remove contaminated sections and replaced with new layers. Over three coats of a dense paint called "Prufcoat" a thin, rubbery film called "Cocoon" is sprayed, reports Dr. A. Carleton Jealous, of the Oak Ridge National Laboratory. For a small unit, the method takes 48 man-hours of labor, requires five days for drying, and costs $155 at the present experimental stage.

Fire Prevention

Plastic aprons, curtains, decorations, or children's clothing should be kept away from lamps, stoves, or candles to avoid serious burns, reports Florence King, of the University of Illinois College of Agriculture. Plastic film does not blaze rapidly or burn with a "puff," but it can give off enough fumes to make breathing difficult, and even to overcome those in contact with them.

Glue Consultation Service

Borden's Chemical Division is offering free consultation service on the best glue for a specific operation. An *Industrial Gluing Chart,* including a *Glue Data Sheet,* filled out and returned to Borden's, will be analyzed by experts and glue recommendations made.

Steel Bibliography

The American Steel Foundries is compiling a bibliography of steel foundry literature, arranged for a punch-card index system, for use throughout the industry.

All-Purpose Spout

A snap-on pouring spout, according to its manufacturer, can be adjusted to fit liquid-pouring containers 1–1¾ inches in diameter, most metal containers from the size of a quart to a 55-gallon steel drum, ½-gallon and gallon capacity glass jugs, and open-end pipes, faucets, or spigots 1–1¾ inches in diameter. Made of either precoated colored metal stock or aluminum, it will fit both screw and cork-type containers.

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THE SCIENCE OF CHEMISTRY

By George W. Watt and Lewis F. Hatch, The University of Texas. 566 pages, $4.50

- This text presents adequate and interesting information on chemical science and technology, for students who are not chemistry majors. The various fields of chemistry are covered, with an especially thorough treatment of organic chemistry. Emphasis is on fundamentals. Provides a comprehensive understanding of the science.

FIELD MANUAL OF PLANT ECOLOGY

By Frank C. Gates, Kansas State College. *McGraw-Hill Publications in the Botanical Sciences.* 138 pages, $3.00.

- Gives directions for exercises and experiments which will enable the student to gather data that can be used to describe various types of vegetation, and also enable him to study individual plants in relation to their environment. The use of "quadrats" and other recent sampling methods is described, and a chapter is devoted to the organization of papers based on field research. Basic directions for suggested exercises are given with unusual simplicity and exactness.

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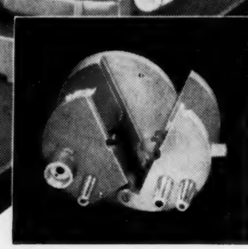

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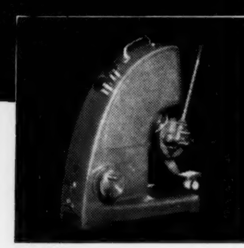

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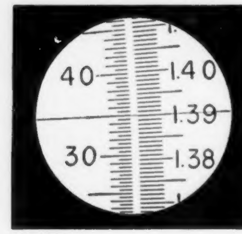

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THE SCIENTIFIC MONTHLY

JUNE 1949

WALPURGIS WEEK IN THE SOVIET UNION

ROBERT C. COOK

Mr. Cook has been managing editor of the Journal of Heredity *since 1922. He was treasurer of the Ithaca Genetics Congress (1932), where Nicolai Vavilov participated as the last Soviet delegate to attend an International Genetics Congress, and he is secretary of the Committee on Displaced Geneticists of the Genetics Society of America. Mr. Cook's article is a condensation of a much longer and more detailed discussion that will appear in the July issue of the* Journal of Heredity.

AT THE Sixth International Congress of Genetics at Ithaca, in August 1932, the late Nicolai Vavilov included in his address to the Congress the following "flash" regarding a discovery made by a Ukrainian colleague:

> The remarkable discovery recently made by T. D. Lysenko of Odessa opens enormous new possibilities to plant breeders and plant geneticists of mastering individual variation. . . . The essence of these methods, which are specific for different plants and different variety groups, consists in the action upon the seeds of definite combinations of darkness (photoperiodism), temperature and humidity. This discovery enables us to utilize in our climate for breeding and genetic work tropical and sub-tropical varieties. . . . This creates the possibility of widening the scope of breeding . . . to an unprecedented extent, allowing the crossing of varieties requiring entirely different periods of vegetation.

In the light of what has happened since, Lysenko's modest acorn of observation on plant physiology has grown amazingly over a period of seventeen years. It has now borne a very strange fruit: an allegedly new "Marxist-Michurinist" genetics. This is the latest thing in science, or the oldest, depending on how we look at it. Whatever status may be assigned Lysenkoism in the mature hindsight of history, it is unique in one respect: it is the only scientific discipline in existence today whose validity depends, not on experiment, but on certification as to purity and truth, in content and concept, by government fiat.

HISTORICAL BACKGROUND

At Ithaca, delegate Vavilov had extended to the geneticists of the world a cordial invitation to hold the next International Genetics Congress in Moscow in 1937, and preliminary plans for the conference in Moscow were well under way by 1935. About a year later it became clear that something was amiss with these plans when a dispatch to the *New York Times* on December 14, 1936, carried the news that the Genetics Congress had been postponed and that Professors Agol and Vavilov had been arrested. Vavilov's arrest was later denied, but the Congress had been postponed "at the request of a number of scientists who had expressed a wish to extend their preparations for the Congress."

The International Organizing Committee was convinced by May 1937 that the "postponement" constituted in effect a cancellation. It was stipulated by the Soviet government that if the Congress were to be held, no papers on human genetics could be presented. Under such circumstances a free scientific meeting was hardly possible, and arrangements were made to hold the Congress instead at Edinburgh, Scotland in 1939. Vavilov was elected its President, which position he did not fill, as no Russian delegates were present. Then in September 1939, while the Congress was in session, the nonaggression pact between Germany and Russia was signed and World War II began.

In the light of what has happened since, it may be significant that the "genetics furore" in the USSR toward the end of 1936 marked the beginning of the blood purge of generals and officials that continued through the following year. In 1938, a genetics mass meeting in Moscow had developed into a runaway Lysenko lovefeast. It is now clear that Vavilov was then standing on the brink of a precipice. Two years later he was suddenly relieved of his post as head of genetic research in the USSR. He died mysteriously in Siberia in 1942.

Vavilov, his prestige based on a superb job of analyzing the origin of cultivated plants, was chosen to organize genetic research by none other than Nicolai Lenin himself. Even this could not save him. Nor was he the only martyr. Among the distinguished geneticists who have disappeared are Agol, convicted in 1939 of "Menshevist idealism," and Levit, Director of the Human Genetics Institute. That other prominent "classical" geneticists were able to continue their work was due, Lysenko has made it very clear in his recent speeches, to forces he was not yet powerful enough to cope with. The battle of genetics in the USSR has not been fought so much in the laboratories as in the Congresses, dominated by laymen, and in the executive meetings of the Politburo.

Vavilov's successor as President of the All-Union Lenin Academy of Agricultural Sciences was Trofim Lysenko, one of the youngest members of that august Academy, and surely one of the least well educated. A practical plant breeder, with a minimal knowledge of experimental genetics, Lysenko still faced serious opposition. Science in the Soviet Union was at that time under control of the Academy of Sciences, which was then at the peak of its prestige. The mean age of the academicians, Eric Ashby tells us, averages about sixty-five years. These "conservative aristocrats of Soviet science" have always looked askance at Lysenko's weird theories. Ashby was convinced in 1945 that "Lysenko and his school were quietly tolerated" because of his popularity with the politicians, but that he had already passed his zenith. Ashby seems to have underestimated the extent to which Lysenko's ideas appealed to the top Communists, as well as to the farmers. His power was growing, and, sponsored by No. 3 Communist Malenkov, great things were in store for him.

The next step in the drive for "Marxist-Michurinism" was the publication in 1946 of Lysenko's *Heredity and its Variability*. Following closely on definite information that Vavilov was dead, this book inspired several critical articles in British and American scientific journals. The reply to these appeared on September 2, 1947, in a leading article in *Pravda* attacking certain "reactionary" biologists in and out of the Soviet Union. Academician Zhebrak, taken to task for a lack of loyalty to "Soviet biological ideas" and for consorting with such "open enemies of the Soviet people as Dobzhansky and Timoffief-Ressovsky," was replaced as President of the White Russian Academy of Science by N. I. Grashechenkov, a party propaganda expert.

Genetics Congresses seem to be nodal points in the history of Michurinist genetics. To the Seventh International Congress of Genetics in session in Stockholm, July 7–14, 1948, came word from the Soviet Academy of Sciences that "The Russian geneticists are too busy to leave their work;" hence, they would not be present at Stockholm.

The nature of this "work" was revealed three weeks later when the Lenin Academy of Agricultural Sciences met in Moscow on July 31–August 7. Like that historic meeting in 1939 when Vavilov suffered his first defeat, this gave the illusion of being a valid scientific discussion. Professor Zavadovsky, *doyen* of Russian animal geneticists, let the cat out of the bag when he outspokenly criticized the organization of the meeting. A sick, ailing old man, going from one sanatorium to another, Zavadovsky had heard of the meeting by accident.

> Insufficient opportunity was given those who are rightly, and especially for those who are wrongly considered among the Weissmann-Morganists, to prepare and to have the possibility to express themselves freely and fully. . . . We are making a big mistake [when] those who dare not agree with Lysenko are in a wholesale manner put by Lysenko's supporters in the odious category of "formal geneticists" . . . I consider that this narrow, limited, one-sided line of slandering not only the methods but the people who are not working under the approved plan is an inadmissible thing. . . . Who gives the right to include under the name of Darwinism that context which contradicts his teachings?

Others—notably Zhebrak, already in disgrace and under heavy fire—spoke eloquently in support of their views. The minority of Mendel-Morganists were frequently interrupted and ruthlessly heckled, mainly by Lysenko and Prezent. Those who disagreed in any respect appeared to be cast in the role of defendants in a court rather than of scientific workers engaged in a search for the truth. A reading of the transcript of this meeting makes it clear that a Thomas Committee on Un-American Activities and a Lysenko scientific gathering have many things in common. Lysenko had the advantage of Thomas, who might say to a witness: "The rights you have are the rights given you by this committee." Thomas' on-the-spot "rules" were subject to review by the courts. Before the chairman opened the first session of this "scientific" gathering in Moscow, an important

decision had already been made, and was reposing in Lysenko's pocket. What this was the assembled academicians would not know until they had committed themselves. Then an announcement would be "forced" from a coy Lysenko on the tenth and last session, to explode the bomb.

The discussions were over, and Lysenko again had the floor to sum up. In sixty words—and in one of the most straightforward and understandable sentences Lysenko ever penned, the pay-off was announced:

> Before I pass on to my concluding remarks I feel it my duty to make the following statement:
>
> The question is asked in one of the notes handed to me, "What is the attitude of the Central Committee of the Communist Party to my report?" I answer that the Central Committee of the Party examined my report and approved it.

The answering of these questions so conclusively will surely have a place in history. Political authority, technically incompetent to evaluate the data of scientific research, had decided vital questions of fact on which a scientific discipline depended. Many basic biological tenets, proved and accepted elsewhere in the world, were false and "heretical" in Russia. Lysenko was making his own rules.

What happened to Zavadovsky we do not know: we hope he was allowed to continue his trip to the new sanatorium. A chasm had opened before more active and prominent scientists, Zhukovsky, Schmalhausen, and others, who still held places of prominence in the scientific hierarchy. Immediately after Lysenko finished speaking a strange parade of recanters began. Three academicians recanted on the spot. P. M. Zhukovsky, who had expressed an ardent belief in the importance of chromosomes and had doubted the validity of Lysenko's "experiments," now volunteered "to fight—and sometimes I am capable of fighting—for Michurin's teachings. I am working for the Committee for Stalin Prizes, and in the Council of Ministers, and therefore I think that I have a great moral duty—that is to be an honest Michurinist, to be an honest Soviet biologist." Said Academician S. L. Alikhanian: "As a Communist, I cannot and must not pit my personal views and understandings against the course of development in biological sciences." And Academician I. M. Poliakov: "The only thing for party and non-party workers to do is to say right out that Michurinist direction is the general road of development of biological science. . . . You must understand that this [foreign] rottenness has influenced some Soviet scientists, and it is necessary to eradicate it to the end. I will work for Lysenko."

During the following week hardly an issue of *Pravda* appeared which did not carry the craven renunciation of a lifework by some of the great names in the once fine and respected field of Soviet biology. Then on August 26 the All-Union Academy of Sciences of the USSR met, and the official heads began to roll. In an open letter to Stalin, the Presidium of the Academy promised "resolutely to correct the mistakes we have made, to reorganize the work of the section on biological sciences and its institutes, and to develop the biological sciences in a genuine Michurinist direction."

None of these changes in attitude were predicated on the question of the validity of the evidence. The basic data never even came up for discussion. Faith, as defined by a perplexed schoolboy as "belief in something you know is impossible," became the basis for biological science in the USSR. "Personal views" congruent with the relative data, must now bow before the official line of the party, however absurd that line might be. "It still moves," tradition tells us the recanting Galileo whispered as they released him from the stake. "The genes still segregate at random, in spite of Lysenko and Malenkov," we can hear these frightened people whisper.

Into official limbo went the Secretary of the Biological Sciences Section, L. A. Orbeli, an outstanding student of Pavlov, a Stalin Prize Laureate, and the leading physiologist of the Soviet Union; and Academician I. R. Schmalhausen, distinguished Director of the Palaeontological Institute. M. B. Dubinin, the eminent *Drosophila* geneticist not only was discharged as Director of the Institute of Cyto-Genetics—the institute itself was abolished amidst sarcastic screamings in *Pravda* against "such incompetents playing with fruit flies"!

Orbeli's successor, A. I. Oparin, promised that all experimenters in natural science would reconstruct their work in a fundamental fashion and cease "fawning and servility before foreign pseudo-science."

This resounding defeat to the "aristocracy of science in the Soviet Union" was administered to an Academy headed by Sergei Vavilov, the brother of geneticist Vavilov, liquidated in 1942. Said physicist Vavilov: "Our mistake has been primarily to fail to see that one of the conflicting trends, the Michurinist teaching, is genuinely materialistic and progressive. The organistic type of Mendelian trend is idealistic and reactionary."

On August 27, 1948, a *Pravda* editorial, quoted in the official *Soviet News,* put the capstone on this weird nonsense in explicit and all-inclusive terms:

> The Presidium of the Academy of Sciences and the Bureau of the Biological Department *forgot the most*

important principle in any science—the party principle. They pegged themselves to a position of political indifference and "objectivity." The U.S.S.R. Academy of Sciences forgot the instructions given by V. I. Lenin that "partisanship" is inherent to materialism, and that materialism, whatever phenomena are being considered, must stand openly and directly on the viewpoint of a definite public group.

Walpurgis Week was over. The "pseudoscientific," "reactionary," "idealistic" biology of the capitalist world was dead.* *Pravda* and *Izvetzia* screamed approval. The country boy from the Ukraine who came to Moscow less than ten years before had made very good indeed!

BASIS OF LYSENKO'S CLAIMS

What, precisely, is this amazing new genetics of Lysenko? It is said by many leftish writers in the United States that Lysenko's "calmness of tone, scholarly approach, and patiently marshalled facts" demand recognition for his views. A feature article in the leftish *Masses and Midstream* in March 1949 says, "This controversy will affect biological science as profoundly as did Darwin's theory of natural selection, which was also highly controversial in its time."

Is the universal outcry against Lysenko's views by competent geneticists motivated by a desire to conceal the truth and to prevent the emergence of a powerful new organon? Is it possible, merely by paying $1.25 for a copy of Lysenko's *The Science of Biology Today* to "read the facts in the Lysenko controversy," as claimed in an advertisement of this book?

We do not have space here to consider the details of Lysenko's claims. They have been reviewed most carefully and fully by two competent geneticists.† Lysenko makes twelve didactic basic claims regarding genetics which are false, unprovable, or unproved. For example, he makes the flat assertion that "a hybrid F_1 plant is never later than the early parent." This is untrue, and is so proved by a mass of evidence. When Lysenko states in his latest book that "any character may be transmitted from one strain to another by means of grafting as well as by the sexual method" his only "evidence" is some puerile "experiments" that would shame a high-school boy. When Lysenko says "You need but change the type of metabolism in a living body to bring about a change of heredity," he adduces no clear evidence to support this claim, and he ignores the overwhelming weight of evidence against the proposition. This evidence stands solidly athwart his claim that "We can change heredity so as fully to meet the effect of the action of conditions of life." When Lysenko says "We must firmly remember that *science is the enemy of chance* [italics his] . . . "That all the so-called laws of Mendelism-Morganism are based entirely on the idea of chance . . . does not deserve to be called science," that "physics and chemistry have been rid of fortuities. That is why they have become exact sciences," he is obviously talking utter nonsense.

The words may be "calm in tone," they may have a "scholarly" ring to the layman, but they are the words of magic, of ex cathedra assertion, uttered by one who at best is a sincere and misguided bigot and at worst an utter charlatan. But they are not the words of science. To take Lysenko's statements one by one, and to evaluate them and extract the iota of truth from the few that are not completely false, would take pages. As Richard Goldschmidt puts it: "Such elementary facts as the chance assortment of chromosomes Lysenko considers to be mythical nonsense. . . . How is it possible he has never taken the trouble to see with his own eyes what thousands of students all over the world are unfailingly shown?" No, the apologies of the Fasts and the Spitzers, even the inspired obscurantism of J. B. S. Haldane (who ought to know better), cannot make a scientist out of Lysenko, or make anything very useful out of his mystical "Michurinism."

If this is not science, what, then, is it, and why has it captured the imagination of the masters

* A story attributed to the Associated Press states that a refugee to the U. S. Zone of Germany reports that Schmalhausen, Dubinin, and Orbeli have been liquidated. This has no confirmation.

† For an understanding of this situation, three sources are indispensable. The most complete review of the background and history of the genetics controversy in the Soviet Union was published in 1946 by P. S. Hudson and H. R. Richens, of the School of Agriculture at Cambridge, England. Dr. Eric Ashby, formerly chairman of the Australian National Research Council and at present professor of botany at the University of Manchester, spent 1945 in Moscow on a scientific mission as Counselor at the Australian Legation. His very sympathetic and analytical firsthand account of Soviet Science (*Scientist in Russia*, Pelican Books, 1947) deserves a wide reading in this country. For a more impressionistic analysis of the concept of "heresy" in present-day Soviet Russia, the reader is referred to Humphrey Slater's *The Heretics* (Harcourt, Brace, 1947), which traces a vivid and frightening parallel between the Albigensian Crusade of 1209–11 and the fate of "liberal" Communists in the International Brigade during the Spanish Civil War. What has happened to biological science in the Soviet Union remains a complete enigma until we sense this strange Muscovite obsession with authority and with heresy, which runs like a red thread through this controversy.

of the Kremlin? Lysenko has capitalized on a quirk of character, not confined to the Russians, which delights in dramatized struggle, especially if it is based on magic and buttressed by authority. Richard Goldschmidt has told of the weird propaganda film *Salamandra*, which he saw in Moscow in 1929 and which canonized Kammerer and the inheritance of acquired characters, picturing poor Kammerer as the victim of a fantastic capitalist-clerical plot. This was long before anyone had ever heard of Lysenko. Michurin, the central figure in the strange iconology of Lysenko's genetics, combines these three obsessions. He proposed to "wrest security from nature," and what is essential to his needs is "true." He ardently believed in a childishly naïve inheritance of acquired characters, and he was a "practical peasant" with a profound contempt for "theorists." Burbank, capitalism's Michurin, had, as American biologists know, a "green thumb," an inflated ego, a flair for weird statements, and a contempt for plodding experiments. Zavadovsky made it clear what Lysenko's school had done to Darwin.

This ardent belief in authority, in the magical properties of "necessity," and of the magic power of struggle, long antedates Lysenko and gives him his power. The appeal of the strong-arm approach to science is implicit in Stalin's dictum: "There is no fortress the Bolsheviks cannot take by storm."

Lysenko utilizes, in his "dialectical materialist" approach to heredity, an organon that the Western world had struggled to outgrow since the Middle Ages. Experiment and the use of mathematics are interdicted in this "home-grown" Soviet science. In their stead we have the dialectical antithesis, authority and heresy. The iconology of Lysenko's biological cathedral is decked with the usual Communist apostles—Marx, Lenin, Hegel—and the chapel in which Lysenko carries on his devotions features a strange pentathlon indeed: Darwin, Timiryazev, Michurin, Burbank, and Lysenko himself; the "heretics" and "heresies" are led by Weismann, Mendel, Morgan, "mathematics," "idealism," "vulgar materialism." To label any "heresy" "Weismannism" or "idealist" precludes the need to disprove it experimentally.

Control of nature by understanding natural processes is interdicted. Michurin taught that we must "wrest secrets from nature," just as the Arctic is being "wrested" (with considerable futility) back into the Temperate Zone. Authority (which is absolute), heresy (which is always doomed in the end to fail before authority), the necessary rightness of what must be—these concepts take the place of experiment, and make experiment unnecessary. This is not science, it is the ancient magic and ancient authoritarianism the human race has struggled so long to escape.

There is increasing evidence that the virus of medieval obscurantism is extending far beyond genetics—even beyond biology—and in several directions. As long ago as August 1946, the Central Committee of the Communist Party accused social scientists, in particular, and the majority of scientific workers, in general, of being "backward." "The scientific worker is a public worker: he cannot be apolitical. He must guide himself toward the policy of the Party, which reveals itself to be the living basis of Soviet Society." It is also reported that the Party Central Committee has set up a new Academy of Sciences, independent of the existing Academy, whose "backwardness" has been censured by Stalin personally. This is the more strange because in the summer of 1945 the Soviet government celebrated, with great international fanfare, the 220th anniversary of that same Academy, founded by Peter the Great.

In recent months, these criticisms of scientific workers have run the entire gamut from atomic physics to sociology. Since the August reorganization, four atomic scientists have been harshly criticized by Soviet newspapers for the statement that science cannot predict the behavior of atomic particles. On January 26, 1949, A. A. Maximov, of the Institute of Philosophy, attacked, over the Moscow radio, foreign physicists who were "responsible for idealistic interpretations in relativity and the quantum theory." Einstein, Bohr, and Heisenberg were guilty of "Kantian acrobatics," Joliet-Curie, Blackett, and Haldane were praised for their sound doctrine.

The Varga incident of several months ago is typical of these widespread attacks. Varga, a leading economist, was so undialectic as to have made the disturbing suggestion in 1948 that the impending "collapse" of the United States might not come off according to the Marxian schedule. This position branded him as a capitalist-reactionary; he was violently attacked and removed from his job as Director of the Institute of World Economics of the All-Union Academy of Sciences. He continued the argument, however, and was allowed to state his views with considerable freedom. Now an Associated Press dispatch of March 15, 1949, tells us that Varga has recanted. The U. S. is going to "fold," Varga now agrees. In fact, it pretty much already has! Varga's recantation came one day after the announcement that N. A. Vognesensky had been relieved of his duties as head of the Soviet Social Planning Commission.

In medicine three leading scientists have re-

cently been fired from key posts, among them, C. F. Gause, Russia's best-known authority on malaria. At the Academy meeting of August 26, the Minister of Health criticized the reactionary attitude of Davidenkov, Gurvitch, and Rubenstein. The geographers also had a going-over in August 1948. "Pseudo-scientific conceptions, bourgeois in origin" were noted, and their elimination promised. Several dispatches have told of plans to rewrite the encyclopedia along Marxist-dialectical lines!

The die has been cast in the USSR. For better or for worse, the Soviet people are stuck with Lysenko's genetics. If the numerous "straws" of revolt against "capitalist-bourgeois" science mean what they seem to mean, then a home-grown science as broad as the encyclopedia is very busily in the making. The fortress which the Bolsheviks are going to storm this time is not just the Arctic, or the chromosome: it is the entire field of human knowledge. What *is,* painfully established by generations of experiment, must bow before what *must be,* to fit the preconceptions of the weird Procrusteans in the Kremlin!

The recent shifts in the Soviet high command have been analyzed by one authority as being dictated by a newly strengthened antiforeign "axis" centering around Lysenko's sponsor Malenkov, Andreyev and Popov, in the Politburo, and Alexandrov, top party propagandist. This analysis of the situation is fully congruent with developments in Soviet science since last August. This would appear to be a golden opportunity for fervid, bigoted, and convincing "mediaeval obscurantists" of the Lysenko type! The encyclopedia they come up with will make fascinating reading.

In the long run this bizarre attempt to give a dialectical bum's rush to reality is bound to fail, as such attempts have always failed in the past. Even in the short run it is most unlikely to produce any dramatic results. We might be tempted to relax and let "nature" take its course beyond the Iron Curtain, for this trend is surely in our favor. But there is a problem facing this country, and all the hopefully called "free world," that is far broader than the ultimate fate of Lysenko and his queer necromancy.

We would be well advised to give serious consideration to the Soviet predicament. Seven hundred years ago Friar Roger Bacon set forth the first crude code of scientific behavior, dictated by the urgent necessity "to keep from fooling ourselves," to keep the questions we put to nature relevant and in a form allowing unambiguous answers. Science advances by understanding the forces of life and of the universe, not by attempting to dominate them by magic. We are not so far advanced ourselves in practicing this exigent art that we can safely be smug about our own somewhat precarious position. Magic, authority, and wishful thinking are still with us—are still a danger.

All too often in our own country, and in the Western world generally, we are still tempted to ascribe to science and the scientist the role of the priest or the magician. It is not too difficult to understand the Russian obsession with "what must be" when we see it so often nearer home. Scientism very easily becomes the dead hand of preconceived authority. Whether it be in this country or in the Soviet Union, the scientist who pretends to speak with the voice of authority concerning subjects on which he lacks competent information becomes an ally of this, our greatest enemy. For the scientist, our constitutionally guaranteed freedom of speech has a special and a critical meaning. It is—and it must always be—a freedom *to speak the truth in so far as we see it.* It is also the freedom to speak against antitruth (better, "antireality") wherever we find it. For any scientist to speak nonscience, and to use the prestige of his scientific position to expound nonscientific views, is to be guilty of the ultimate treason in the long battle to free the human mind.

The fight against revealed authority, against enthroned opinion, and against the use of power to force acceptance of *ad hoc* assumption as "revealed truth" is by no means ended. It goes on here, as it must go on "underground" in the Soviet Union if the minds of that fine courageous people are ever to be free. In an uncertain world, we are hardly likely to find perfection anywhere, and it is stupid and chauvinistic to claim perfection here. In the vivid phrase of DeWitt Wing, things are "less worse" some places than they are others, and we are very fortunate in many respects. But all who are the intellectual descendants of Galileo, of Servetus, of Vavilov, must never forget that the fight is not yet over. This fight has always been to a finish. The enemy is here as well as in Moscow. Our Spitzers, our Fasts, our Haldanes, and Blacketts, our scientists who pontificate without adequate knowledge, our trustees and executives who engage in mass witch hunts, all these give aid and comfort to that ultimate enemy of science, and of what we call freedom.

But for the grace of all who have fought for the freedom of the human mind, each of us stood in Moscow on August 7 last, and heard the words of doom: "The Central Committee of the Party examined my report and approved it."

AEROSOLS*

FRANK T. GUCKER, JR.

Dr. Gucker (Ph.D., Harvard, 1925) is chairman of the Department of Chemistry at Indiana University. He has taught also at Haverford, Harvard, and Northwestern, and pursued research studies at the California Institute of Technology and the Du Pont Ammonia Corporation.

CLOUDS in the sky and fogs covering the face of the earth are made up of small, invisible drops of water suspended in the air, whereas smoke from the chimneys of our homes and factories and dusts of all sorts consist of tiny solid particles floating in the air. Clouds, smoke, and all air-borne suspensions of minute solid or liquid particles are classed as *aerosols*—perhaps the least-well-known type of colloidal system, although they cause the blue of a summer sky and the red hues of sunset.

My own introduction to aerosols came through work with the National Defense Research Committee during the second world war. In order to test gas-mask filters, new methods of measuring extremely small quantities of aerosols were needed, and these we helped to develop in our Laboratory. Later, in order to guard against the passage of single particles, we worked out automatic methods of counting such colloidal particles. These new techniques may be applied to the study of such problems as the production of dust-free air in factories manufacturing biologicals, the spread of air-borne infections, and dust and smoke contamination in cities. We are continuing the development of methods of determining the number, size, and shape of aerosol particles, in order to gain a better knowledge of these systems.

How are aerosols formed? What factors influence the length of time the particles remain suspended in the air? How may they be removed from the air? What interesting and useful physical properties do aerosols show? How can we determine the infinitesimally small amounts of suspended material they contain, and the minute size and astronomical number of these particles? The answer to these and other questions will give us a better insight into the nature of these fascinating colloidal systems and some of their many practical applications. In some ways aerosols resemble other colloids, and some of the methods of studying them are adapted from the field of liquid-dispersed colloidal systems; others have been borrowed from the biologist and the bacteriologist. This debt may be repaid by the new techniques in aerosol study, which will prove useful in other branches of colloidal chemistry, in the life sciences, and in industry.

Like all colloids, aerosols may be formed either by the subdivision of matter in bulk or by the condensation of molecular dispersions. Solids may be ground mechanically into air-borne dust, some of which is fine enough to remain suspended as an aerosol. Similarly, the toxic smokes used in the first world war were dispersed by an explosive charge which pulverized the irritant material. Wind storms and volcanic explosions in nature may form aerosols on a global scale. In August 1883 the whole top of the volcano Krakatao, on the island of Sumatra in Sunda Strait, was blown off in a series of terrific explosions. A tremendous pillar of dust was thrown into the air. The coarser particles fell upon ships in the Indian Ocean, 1,100 miles away. A cloud of the finer particles was thrown to a height of 50–100,000 feet, where the prevailing westerly winds carried it more than twice around the world. It spread out in all directions and blanketed the continent of Europe, far above the rain- and snowstorms which clear the dust from the lower atmosphere. For two and a half years this fine dust in the upper atmosphere caused brilliant sunrises and sunsets, in which at times the disk of the sun appeared blue or green in color.

On a somewhat smaller scale, fine desert dust from Africa has been swept aloft by the winds, carried 2,000 miles over the Atlantic Ocean, and deposited in England and on the continent of Europe. Dust from the central desert of Australia has been carried 1,500 miles over the Pacific to fall in New Zealand. In this country, during the droughts in the thirties, high winds swept the dust of the Western states aloft to heights of 5,000–10,000 feet, carried it 1,200–1,800 miles in twenty-

* The sections of this paper describing the work of the author and his collaborators are based in part on work done for the Office of Scientific Research and Development under Contract OEMsr-282 with Northwestern University, and in part on work done for Camp Detrick, Maryland, under Contracts WA-18-064-CWS-137 and -160 with Northwestern University, and Contracts W-18-108-CM-31 and W-18-064-CM-218 with Indiana University.

four hours, and covered whole states with a dull haze. Dust from north Texas was collected in Buffalo, New York, and that from the Dakotas fell on the Eastern seaboard. The collected dust ranged from 1 to 50 microns (thousandths of a millimeter) in diameter, with the most common size usually less than 10 microns.

Many wind-pollinated plants also form widespread aerosols, and the sufferer from hay fever may sneeze from the pollen of ragweed blooming hundreds of miles away. Frequently, plant diseases also are broadcast by spores dispersed as aerosols. E. C. Stakman records the rapid spread of an epidemic of wheat stem rust by spores carried a thousand miles in forty-eight hours; and there is evidence for a yearly cyclic migration of this disease, from Texas to points as far north as Manitoba. The rust in the North does not survive the winter, but the infection is reintroduced each summer from the South. There the rust is burned out each summer, and returns from the North in the fall. Thus the air-borne migration of the wheat rust is comparable in scope to that of birds. A similar cyclic migration of this disease occurs in India, between the hot plains and the cool mountain slopes.

In addition to mechanical subdivision of solids or liquids, aerosols also may be made by spraying a solution of a solid substance in a volatile solvent, which evaporates to leave small dispersed particles. For example, when the crest of an ocean wave is whipped off by the wind, the fine drops quickly evaporate to leave tiny salt crystals, which are carried by the wind almost all over the world. The same thing occurs when a solution of DDT, a solid, is sprayed from a "bug bomb" to form an aerosol lethal to insects.

Solids colloidally suspended in liquids may be converted to aerosols by spraying them into the air as fine droplets from which the liquid evaporates to leave the individual solid particles. This is the familiar method by which the victim of a cold, sneezing violently, sets up an aerosol consisting partly of air-borne viruses and bacteria which spread the disease. Figure 1 is a photograph of a sneeze, taken by Marshall W. Jennison, of MIT, using Edgerton's flash illumination of 33 microseconds' duration to "stop" the motion of the particles. More than 40,000 particles have been counted in such a photograph, and an average of over 20,000 microorganisms has been demonstrated in the air by W. F. Wells as the result of one good sneeze.

Fogs and other liquid aerosols are formed when the vapor is chilled below the dew point. Condensation occurs most readily around fine particles of dust, ions, or other nuclei in the atmosphere; hence, fine smoke particles in the air of cities help condense the water vapor to "smog." Similarly, the ions formed by radioactive rays act as nuclei for condensation in the Wilson cloud chamber used in studies of these rays.

Most of the physical properties of aerosols depend upon the size, mass, and number of the particles they contain. Thus, aerosols containing large, dense particles soon settle out and disappear, and those with a high concentration of particles tend to coagulate rapidly. Just as the study of hybrid plants requires uniform purebred lines, so the quantitative study of the properties of aerosols requires the production of homogeneous samples. Striking and beautiful optical effects shown by homogeneous aerosols may disappear completely if some of the particles are of a slightly different size. The efficiency of the screening smokes used for the protection of cities during the second world war depended largely upon their particle size. So also does the efficiency of insecticidal aerosols, and the ability of medicinal aerosols to penetrate, and remain in, the bronchial tubes.

Most naturally-occurring aerosols are heterogeneous, containing particles of many sizes. Although it is hard to produce homogeneous solid aerosols of any but the smallest size, homogeneous liquid aerosols can be produced over a considerable range by a method developed in 1941 by Victor K. LaMer and David Sinclair, working for the National Defense Research Committee. They saturated a stream of air with the vapor of a high-boiling liquid, diluted and superheated it somewhat, then passed it through a tube, where it cooled slowly and thus condensed to form a homogeneous aerosol. They found that if the diluting air was passed through an electric spark or over a heated salt it appeared to pick up condensation nuclei that improved the uniformity of the aerosol. The size of the particles was controlled by varying the amount of vapor in the stream. As soon as the homogeneous aerosol was formed, it was diluted with a large volume of air at room temperature in order to stabilize it and prevent it from changing further. LaMer and Sinclair used these aerosols to determine the influence of particle size upon the completeness of the removal of the aerosol particles in gas-mask filters, upon their light-scattering properties, and upon their efficiency in cutting down visibility when they were used as screening smokes.

It is no easy matter to determine the number, size, and shape of aerosol particles. If they are

large enough, it is possible to allow them to settle on microscope slides, where they can be counted or measured with suitable magnification. Simple solid particles may be examined satisfactorily thus, although more complex filaments may be distorted. The particles in liquid aerosols tend to coalesce if they touch each other, and to spread on the slide glass, unless the surface is treated with a special film that is not wet by the liquid. Under the most favorable conditions, microscopic examination with the highest practical magnification does not allow particles under the microscope, and allows an approximate estimation of size distribution from chemical or colorimetric analysis of the amount of material collected on each slide. This instrument functions most efficiently with particles from a few tenths to 50 microns in diameter.

The principle of the impinger has been applied ingeniously by Owens in his jet dust counter, used in industrial surveys. The sample of aerosol is collected in a cylindrical tube lined with a sheet of moist blotting paper. In the bottom of the tube is a

FIG. 1. Aerosol produced by a sneeze. (Courtesy of Marshall W. Jennison.)

measurements below about 0.4 micron in diameter.

In sampling coarse aerosols, frequently it is convenient to divide them into a number of different fractions of approximately uniform size. This may be done by means of a cascade impactor developed by K. R. May and improved by L. S. Sonkin. The device consists of a series of jets of decreasing size, with a sampling slide mounted close behind each. The air stream passes through the jets at an increasing rate, up to approximately sonic velocity, and deposits the largest particles on the first slide, and successively smaller particles on the subsequent ones. This partial size fractionation speeds up the tedious process of determining the size of slit 0.1 millimeter wide, directly above a microscope slide, which is enclosed in a small chamber connected to a suction pump. A quick stroke of the pump draws out the aerosol, and the nearly adiabatic cooling condenses moisture on the particles, increasing their mass and hence their tendency to strike the slide and to stick. The collected particles are studied under the microscope.

Very fine smokes will not settle appreciably upon a slide, and must be sampled by some other means, such as thermal or electrostatic precipitators (which will be discussed later). In the study of such aerosols, the electron microscope is playing an increasingly important role. When a magnesium

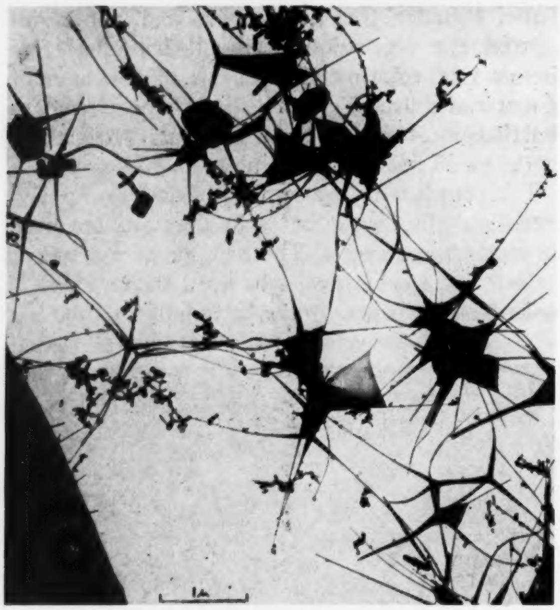


FIG. 2. Electron photomicrograph of zinc oxide smoke. (Courtesy of Farrand Optical Company.)

ribbon is burned in air, the resulting fine smoke of the oxide is produced by condensation of the molecules originally formed. Minute cubic crystals of magnesium oxide, collected from the smoke of a burning magnesium ribbon, are used to test the resolving power of an electron microscope. Another type of solid smoke is zinc oxide, formed when zinc is burned in air. This is illustrated in the electron photomicrograph obtained with a Farrand electrostatic electron microscope, and is reproduced in Figure 2. This shows how dust and soot particles in the home build up to "cobwebs" for which the housewife frequently blames the innocent spider.

The particles in liquid aerosols are much simpler than those in solid aerosols, since they are drawn into a spherical shape by surface tension and thus present the minimum surface for a given volume. Such liquid aerosols may persist far below the usual freezing point of the liquid. Clouds in the winter sky consist of supercooled liquid droplets, which freeze on contact with an airplane to form a layer of ice. Crystallization of the clouds to form snow may be induced by sudden local cooling, as Irving Langmuir, V. J. Schaefer, and B. Vonnegut, of the General Electric Research Laboratory, have shown. From an airplane they sprinkled pieces of solid carbon dioxide in a supercooled cloud to produce man-made snow. They achieved the same results with a supercooled water cloud in a cold chamber, which they "seeded" with very fine aerosols of silver iodide and other crystalline solids. They suggested that small amounts of these substances could be used to induce snowfalls over large or small areas, under the proper conditions.

Returning now to the methods of counting particles, R. Whytlaw-Gray and his collaborators in England have developed a special ultramicroscope cell in which they count the aerosol particles directly, without first precipitating them on a slide glass. The whole arrangement, very much like the standard slit ultramicroscope used in the examination of colloidal dispersions in liquids, is shown in Figure 3. The glass cell *G* is 0.1 millimeter deep and 2 millimeters long, with parallel sides which are made optically flat. The entire space between them is uniformly illuminated by an intense beam of light passing along the axis of the cell. The particles are observed through a microscope objective with a focal depth exceeding that of the cell. Thus the depth of the sample of aerosol is limited by the cell, and the area is limited by suitable stops. The particles appear as bright spots on a nearly black background. By smearing the parallel plates with a thin layer of glycerol, paraffin oil, or some other involatile liquid, particles striking the glass are made invisible, and those in the cell are brightened by condensation. The aerosol enters the cell through a short inlet tube *I* and leaves through an exit tube connected to an aspirator through a stopcock *J*, which is rotated by means of a motor. Thus successive samples of the aerosol stream are drawn into the cell and stopped momentarily under the microscope. The size of the stop diaphragm is chosen to give an average of two or three particles

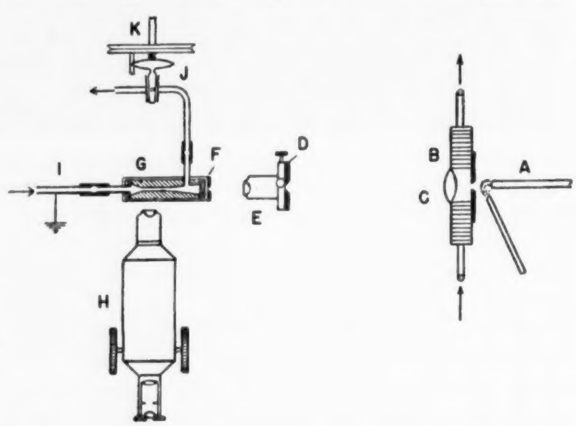


FIG. 3. Ultramicroscope counting apparatus of Whytlaw-Gray and his collaborators. (From *Smoke*, by R. Whytlaw-Gray and H. S. Patterson, Edward Arnold & Company, London, 1932.) *A*, arc; *B*, water-cooled slit; *C*, collecting lens; *D*, slit; *E*, illuminating objective; *F*, slit; *G*, ultramicroscope cell; *H*, viewing microscope; *I*, inlet smoke tube; *J*, rotating stopcock; *K*, wheel carrying arm to rotate stopcock.

in the field of view. In one minute, 60 fields can be counted, giving an adequate statistical average. Since the background light is appreciable with this cell, the method is limited to particles of about 0.1 micron in diameter.

For counting smaller particles, H. L. Green has developed an ingenious method of condensing water vapor upon them during an adiabatic expansion. This is a modification of the familiar cloud chamber developed by C. T. R. Wilson for photographing the tracks of radioactive rays. The expansion chamber is connected to an ultramicroscope cell, and photomicrographs are taken at right angles to the illuminating beam. The apparatus is operated to give 50 photographs in 100 seconds. The particles on the photographs are counted with a low-powered microscope and a calibrated squared graticule. Knowing the depth of the original light beam and the magnification in the process, the particulate concentration is found. Green's method is applicable to particles wet by water, and could be extended to others by using different liquids for condensation. In an inhomogeneous aerosol, the smallest particles may be missed because condensation is stopped by the latent heat evolved.

How long do aerosol particles remain in suspension, and how do they change as time goes on? We know that colloidal systems dispersed in liquids may remain stable indefinitely. In fact, suspensions of silica are found included in quartz rocks, where they have remained for geologic ages. Aerosols, however, are ephemeral, changing and disappearing more or less rapidly after they are formed. Whenever two particles collide, they apparently stick together to form a larger aggregate, and all the particles tend to settle out of suspension under the influence of gravity. Large particles settle much more rapidly than smaller ones of the same shape. In fact, the rate of settling of spherical particles is nearly proportional to the *square* of the diameter. Thus in one second a water drop 100 microns in diameter would fall 31 centimeters through still air at 20° C, a 10-micron drop would fall 3 millimeters, and a 1-micron drop, only 36 microns. A fog of 100-micron drops settles out nearly nine thousand times as fast as a fog of 1-micron drops. In an inhomogeneous aerosol, the largest particles settle first, leaving the smallest in suspension. The distance which the wind can carry aerosol particles such as dust or bacteria depends upon their size. Thus, if a spherical particle of the density of water and 10 microns in diameter falls from a height of 10 meters, it will be carried 9 miles by a wind of 30 miles per hour, before it touches the ground. Under the same conditions, a 1-micron particle would be carried 900 miles.

One method of measuring the size of aerosol particles 1 micron or more in diameter is to study their rate of settling, with an ultramicroscope. Great care is necessary to avoid thermal convection currents, and the density of the particle must be known, or determined by a separate experiment. H. S. Patterson and R. Whytlaw-Gray have used R. A. Millikan's oil-drop technique. They studied the rate of rise of individual charged particles in an electric field and their rate of fall under gravity. Knowing the charge of the electron, they have used Millikan's equations to calculate both the radius of the particle (assumed practically spherical) and its density. For a liquid (oil) smoke they found the mean density of all the particles was the same as that of the original oil.

The results for solid smokes are strikingly different, as typified by Table 1, which shows the data for magnesium oxide. The normal density of the solid is 3.6. One of the small particles has nearly this density. Most of the others, however, have a much lower density, averaging 0.35, or about one tenth of the normal. That is because the larger particles are loose aggregates of the denser primary particles first formed from the vapor phase. The study of individual particles is tedious and slow, and Whytlaw-Gray and Patterson have photographed an ultramicroscope cell to obtain traces of the paths of many particles, the lengths of which are proportional to the squares of the diameters.

Aerosol particles less than a few microns in diameter settle very slowly, and also show increasingly vigorous Brownian movement. Instead of falling in a straight line, they dance about under the buffeting of the gas molecules, with which they become more nearly comparable in mass. At any temperature, the mean displacement in one second, owing to the Brownian movement, increases inversely with the square root of the diameter of the particle. It equals the settling velocity for particles of unit density and about 0.5 micron in diameter.

A. Winkel and H. Witzmann have made a quantitative study of the Brownian movement to de-

TABLE 1

RADIUS (r) AND DENSITY (d) OF MAGNESIUM OXIDE PARTICLES

r(micron)	0.326	0.493	0.617	0.698	0.835	0.835	0.859	0.936	0.949
d(g/ml)	3.48	0.52	0.42	0.39	0.28	0.24	0.40	0.23	0.30

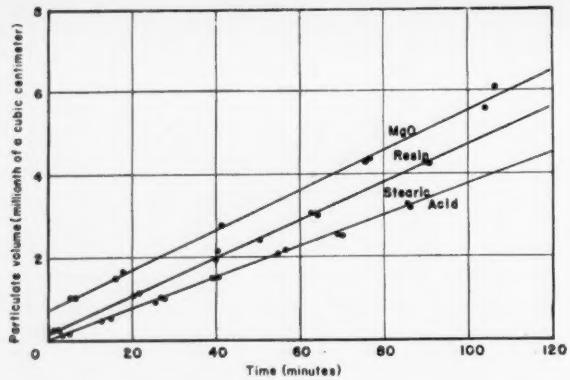


FIG. 4. Coagulation rates of typical smokes. (From *Smoke*, by R. Whytlaw-Gray and H. S. Patterson, Edward Arnold & Company, London, 1932.)

termine the size of these small particles. They used an ultramicroscope with an intense light source interrupted by a rotating sector disk to illuminate the particles with 20 flashes of light per second. A microscope camera, with film moving at a uniform rate, thus took 100 ultramicrographs in 5 seconds, from which the movements of a number of particles were determined. The radii of the particles were calculated from the laws of the Brownian movement.

Another very ingenious method of measuring aerosol particle size was introduced by P. V. Wells and R. H. Gerke. They passed the aerosol slowly between the plates of an electrical condenser and took ultramicrographs of the tracks made by the charged particles, as the field was reversed by means of a rotating commutator at intervals of 0.25–0.5 second. They showed that the diameter of each particle could be calculated from the number of electronic charges upon it, the field strength, the frequency with which it is reversed, and the amplitude of the oscillation. All these factors can be measured conveniently. Wells and Gerke found that a charge was carried by about one third of the aerosol particles formed from oil dispersed in a cubic meter box (apparently by means of a blasting cap). They state that nearly all these particles possess a unit electrical charge, and calculate their diameters on this basis, in the range 0.05–0.7 micron. The method is valid if the particles are representative in size and singly charged. If, as is usually the case, the original aerosol contains few charged particles, some method of imparting a known uniform charge to most of them would be necessary before using this method.

In studying the aging of aerosols, the particles in representative samples may be counted from time to time to determine the number per cubic centimeter, or the *particulate concentration*, the reciprocal of which represents the average space per particle, or the *particulate volume*. As coagulation takes place, the particulate concentration decreases, and the particulate volume is found to increase linearly with the time. Thus the coagulation of a homogeneous aerosol follows the same equation as a second-order chemical reaction. Figure 4 shows plots of some results of Whytlaw-Gray and Patterson for the coagulation of smokes of magnesium oxide, resin, and stearic acid. All three show a linear increase of particulate volume with time, and nearly the same slopes, indicating that the rate of coagulation depends more upon the number than the nature of the particles.

An interesting application of the theory of collisions between gases and aerosol particles has been made recently by Theodore W. Puck to explain the mode of bactericidal action of aerosols of the glycols and other chemical agents. Many had argued that it was due to collision between aerosol particles and the air-borne bacteria. Puck showed, however, that such action would be very slow, and that the observed bactericidal rate must be due to the much more rapid diffusion of the molecules of the vapor to the bacteria. The difference is enormous. Under Puck's experimental conditions, aerosol particles of 2-micron diameter would have required twenty-three hours to collide with and kill 90 percent of the bacteria of 0.6-micron diameter, whereas diffusion of the molecular vapor requires but four seconds for lethal contact.

When an aerosol is fine enough to sediment slowly, its stability depends chiefly upon its particulate concentration. This stability may be measured by the *half life*, that is, the time required for the number of particles to decrease to half its original value. The half life is inversely proportional to the original concentration. If the concentration exceeds about 10 million particles per cubic centimeter, coagulation is rapid and the aerosol is not stable. If the aerosol contains somewhat less than a million particles per cubic centimeter, it changes more slowly and is stable enough for convenient study. Thus ammonium chloride aerosols containing 10 million particles per cubic centimeter have a half life of only two minutes, whereas those containing 100,000 particles per cubic centimeter have a half life of four hours. Aerosols used for laboratory study may contain originally 100–1,000 micrograms (one milligram) of dispersed material per liter. Thus the weight of the aerosol particles is usually less than that of the air in which they are dispersed.

Frequently, the removal of aerosols is a matter

of great practical importance, whether they are the toxic smokes of chemical warfare, dust or fumes from industrial processes, or bacteria. Various methods may be used, depending upon the quantity and type of suspended material and the amount of air which must be purified. Probably the simplest method is filtration.

When an aerosol is passed through a filter of absorbent cotton, paper, or any other material, the efficiency of its removal depends upon a number of factors. In any practical filter, the openings generally are far larger than the aerosol particles, so that the action is unlike that of a sieve. During the second world war, Irving Langmuir developed a theory of mechanical filtration, in which he considered the filter as a series of evenly spaced layers of fine fibers through which the aerosol passes. In respirator filters, the air moves slowly enough so that the flow is streamlined. The larger particles cannot follow the small radius of curvature of the flow lines; hence they collide with the fibers and are removed. The very small particles diffuse across the flow lines because of their Brownian movement and touch the fibers. Liquid aerosol particles of about 0.3-micron diameter are of an intermediate size, which is most penetrating. Extremely fine fibers, such as those of asbestos, supported in a porous paper which prevents them from matting, form a very efficient filter, found in many of the best gas-mask canisters during the second world war.

When a series of identical filter sheets is used, each should remove the same fraction of a homogeneous aerosol. Thus if one sheet allowed one tenth of the original aerosol to pass, a second sheet would remove all but one tenth of the remaining aerosol, or 1 percent of the original material. In general, a plot of the logarithm of the penetration against the number of sheets should give a straight line. This relationship is called the Filter law. Since a small pressure drop is a great advantage, particularly in a respirator filter, the efficiency of a filter is judged by the smallness of the product of the penetration times the pressure drop.

In removing dust from blast furnace, coal, or producer gas, filters may be used, although they become less economical the larger the volume of air and the mass of aerosol that must be handled. For large installations handling hot gases, the method of filtration is impractical, and the Cottrell electrical precipitator is used. Here a high d.-c. potential of 20,000–30,000 volts is applied to give a brush discharge between a series of sharp points and a flat plate. The discharge ionizes the air, and the ions charge the aerosol particles, which are attracted to the plate and deposited very rapidly. Some years ago, in a cement works at Riverside, California, a gas-fired rotary kiln, 7 feet in diameter and 100 feet long, spewed out hot gases at the rate of 50,000 cubic feet per minute. These gases carried along 4 or 5 tons of dust every twenty-four hours, and made the countryside desolate. A Cottrell unit, made of steel, was installed on the top of the 80-foot stacks. The electric current precipitated the aerosol from the hot gas, thus eliminating the smoke damage and at the same time collecting valuable potash. A Cottrell installation at the Washoe reduction works of the Anaconda Copper Mining Company uses more than 100 miles of chains for the points to give the electrical brush discharge. Tons of material, hazardous or noxious as smoke, can be precipitated and recovered in this way, frequently forming a valuable by-product.

A different method of precipitating aerosols is based upon a discovery by Aitken. He noticed that a heated wire, in a flask of smoke, is always surrounded by a clear space, from which the aerosol particles are driven by heat. Thus the establishment of a thermal gradient in an aerosol makes the particles move in the direction of the heat flow. This causes the deposition of dust on the relatively cool wall behind a radiator and the gray streaks frequently visible on the ceiling of a room directly below the roof of an unheated attic. The strips of lath supporting the plaster make a difference in the heat leakage through the ceiling, and the fine dust is precipitated on the cooler strips. When a room is heated by true radiant heat from a fireplace, not a "radiator," the walls are warmer than the air next to them, so that the dust is not precipitated in this way.

The principle of thermal precipitation was ap-

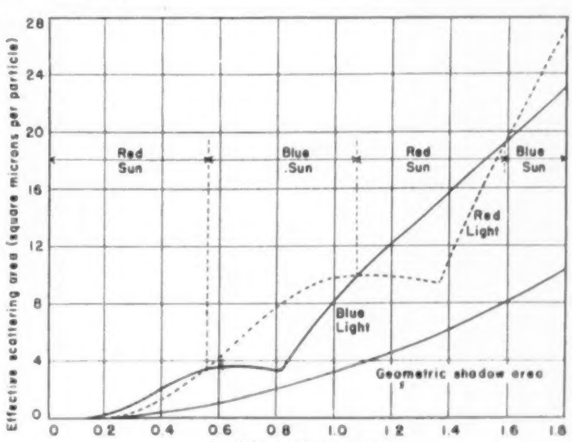


FIG. 5. Effective scattering area per particle of water fog, according to Mie's theory.

plied by Robert Lomax to sampling aerosols by passing them through a narrow space between two parallel plates, the upper heated to 110°C while the lower is kept cool. The aerosol is deposited quantitatively upon a glass slide covering the lower plate and may be examined microscopically. The air stream must be passed through the apparatus rather slowly, so that the principle has not been applied successfully on a commercial scale.

One of the most interesting and beautiful properties of aerosols is the way in which they scatter light. The beam from an automobile headlight is clearly outlined on a foggy night. Similarly, any other colloidal system, in which the refractive index of the dispersed particles differs from that of the medium, scatters some of the light which passes through it. This phenomenon, first noted by Michael Faraday in 1857, was studied later by John Tyndall, and is called the Tyndall effect. Later, Lord Rayleigh calculated the scattering due to very small particles, of a diameter less than one tenth the wave length of the light. He found that such particles should scatter light of short wave length much more than that of longer wave length. Thus he explained the red hue of the sun's disk near the horizon, since red light is little affected, whereas blue light is scattered about eight times as much by fine dust, clouds, and even molecules in the atmosphere. The blue color of the sky he attributed to multiple scattering of the blue part of the solar spectrum by these particles, and his explanation solved a long-standing riddle of nature.

The calculation of the scattering of light by larger spherical particles, comparable in size with the wave length of the light, is a much more difficult problem. In 1908, however, Gustav Mie developed the necessary equations from Clerk Maxwell's electromagnetic theory of light. He showed that the total scattering increases with the size of the particle. The increase, however, is not uniform, and depends upon the wave length of the light and the refractive index of the particle. In Figure 5 we have shown the effective scattering area, in square microns per water particle, for red and blue light. Up to a radius of about a half micron, these particles scatter blue light more than red, as predicted by Rayleigh. Between a half and slightly over 1 micron, the effect is reversed, and red light is scattered more than blue. Viewed through an aerosol of this particle size, the sun's disk would appear blue or green, which probably explains the weird sunsets seen after the explosion of Krakatao. Figure 5 shows another region of particle size over which the color of the sun's disk would appear red, and finally a second blue region.

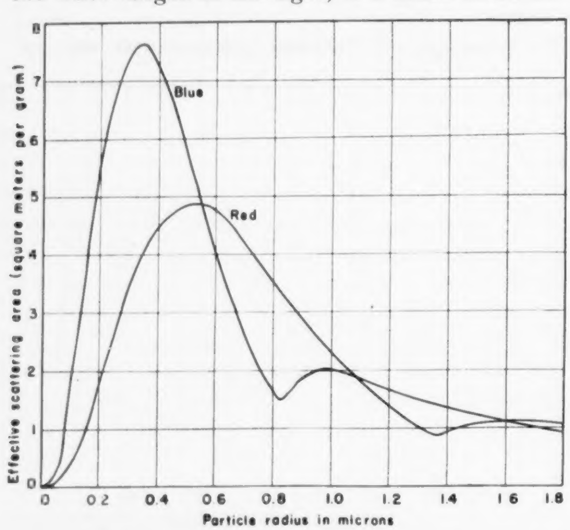


FIG. 6. Effective scattering area per gram of water fog, according to Mie's theory.

FIG. 7. Photoelectric smoke penetrometer.

These curves show that visual observation of the sun's disk, or of any white light, through an aerosol, is a qualitative measure of the particle size, over a considerable range. V. K. LaMer and S. Hochberg, in their study of aerosols for the NDRC, developed these observations into a quantitative method. They passed red and green lights of definite wave lengths through a long cell containing the aerosol, and measured the reduction of intensity photoelectrically. They used the relative absorptions of the two wave lengths to calculate the particle size from the Mie theory, and found that this method is applicable even to aerosols that are not homogeneous.

An unexpected prediction of Mie's theory is the total scattering area for large particles. At first glance, this would seem to be simply the cross-sectional area, which would be true of completely absorbing particles. Either colorless or completely

reflecting spheres, however, show an effective scattering area *twice* that of the geometric shadow. The light which falls upon the particle and is scattered from its original direction *interferes* with the light which grazes the surface of the particle, and diverts it outward through a small angle. Thus the diffraction pattern is analogous to that from a beam of light passing through a small pinhole.

A very interesting relationship is found when we consider the effective scattering area per gram of material, instead of the scattering area per particle. The large number of very fine particles per gram is nullified by their low scattering power. As the size increases, the decreased number is more than compensated by the increased scattering power, until a maximum scattering is reached which is very high and sharp for blue light, and less pronounced for red, as shown in Figure 6. Such considerations show how greatly the particle size influences the obscuring power of water fogs or the screening smokes used so widely in the second world war. They also show why a given aerosol might be a much less effective screen against red or infrared light than against blue light, and would be almost completely ineffective against the much longer radar waves.

Rayleigh predicted that the scattering of light by very small particles is symmetrical, with equal scattering in the forward and reverse directions. He also predicted that the light scattered at right angles to the beam would be completely polarized, and this is true of the sun's light scattered in the earth's atmosphere. As the size of the aerosol particles increases, however, more and more light is scattered in the forward direction, and a new phenomenon appears. A homogeneous aerosol breaks up white light into series of spectra, like those from a diffraction grating. These spectra may appear sharp and brilliant, particularly if they are viewed through a pair of Polaroid sunglasses. This effect was cited by B. Ray in 1921 to explain the "axial colors" he observed in homogeneous sulfur sols.

Observation of the spectra, and particularly of the number and position of the red bands, has been applied recently by LaMer and his collaborators to a problem in chemical kinetics—a study of the growth of sulfur sols. If a parallel beam of white light is passed through a homogeneous aerosol, the number of spectral orders is a measure of average particle size. The orders are determined most easily by viewing the aerosol through Polaroid glasses and counting the number of reds between 0 and 180 degrees of the light beam. For aerosols of all refractive indices, the average radius, in tenths of a micron, is equal to the number of orders, up to about five at least. Interpolations can be made between integral orders. The angular positions of the reds can be measured to one degree, and compared with the positions calculated from the Mie theory. These methods are applicable only to the homogeneous aerosols. In fact, the observation of brilliant spectra is a good criterion of a homogeneous aerosol.

A number of astrophysicists, including H. C. van de Hulst in Holland, have applied the Mie theory of scattering to interpret the changes in stellar spectra due to interstellar dust clouds, and have attempted to determine the chemical nature and size of these particles, which are thought to comprise half the matter in the universe.

The light-scattering properties of aerosols have been utilized recently in developing rapid and extremely sensitive methods of measuring the mass concentration of dilute aerosols (one microgram per liter or less). It is possible to measure either the decrease in the light transmitted through the aerosol, or the amount of light which is scattered.

In 1937, A. S. G. Hill developed an apparatus for testing commercial smoke respirators by means of the difference in light transmission of the test smoke before and after filtration. A strong beam of light passed through a 50-centimeter smoke cell to fall on a photocell, the current from which was amplified electronically. Hill's carbon test smoke, containing 25 micrograms per liter, with an average particle diameter of 0.16 micron, reduced the light on the photocell by only 9 percent, so that the light intensity had to be regulated to two parts in one hundred thousand to achieve a sensi-

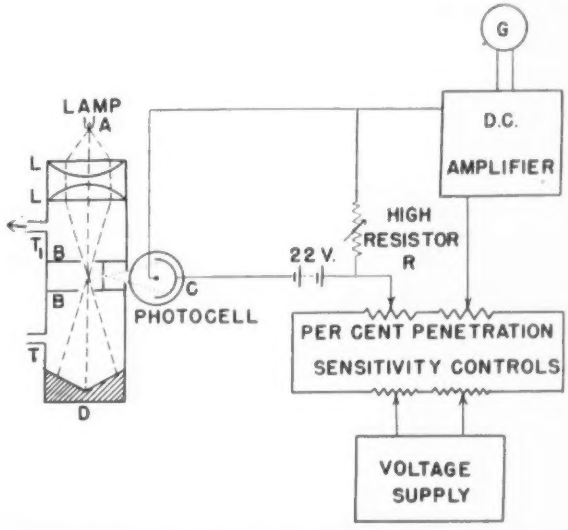


FIG. 8. Schematic diagram of photoelectric smoke penetrometer.

tivity of 5 one thousandths of a microgram per liter, or 2 one hundredths of a percent of his test smoke.

Only a small fraction of the light scattered from the primary beam by the smoke can be collected in an optical system. It can be measured directly, however, and not as a small difference between two large quantities; hence, the light intensity requires no elaborate regulation. A number of instruments have been built for visual comparison of light scattering from aerosols, but these cannot compare in convenience and sensitivity with a photoelectric instrument such as that developed by C. T. O'Konski, H. B. Pickard, and F. T. Gucker, Jr., in our Laboratory at Northwestern University to test gas-mask filters during the second world war. Figure 7 shows the instrument mounted in a 12″ × 20″ × 12″ cabinet. Figure 8 shows the fundamental principles of operation of the photoelectric smoke penetrometer—so called because it was designed to measure the filter penetration of test smokes (usually liquid aerosols). The brass smoke cell is covered on the inside with optically black paper, coated with soot to reduce reflection of light. Smoke enters through T and leaves through T_1. The light from a 50-candle power automobile headlight A is focused in the center of the cell by the two aspheric condensing lenses L, L, and finally absorbed by the V-shaped light trap D. The beam is outlined by the dashed lines. Baffles, B, B, cut off stray light. The light scattered at right angles passes through two vertical slits, to fall on a vacuum photocell C, in series with a 22-volt battery and a high resistor R. The scattered light reaching the photocell is outlined by the dotted lines.

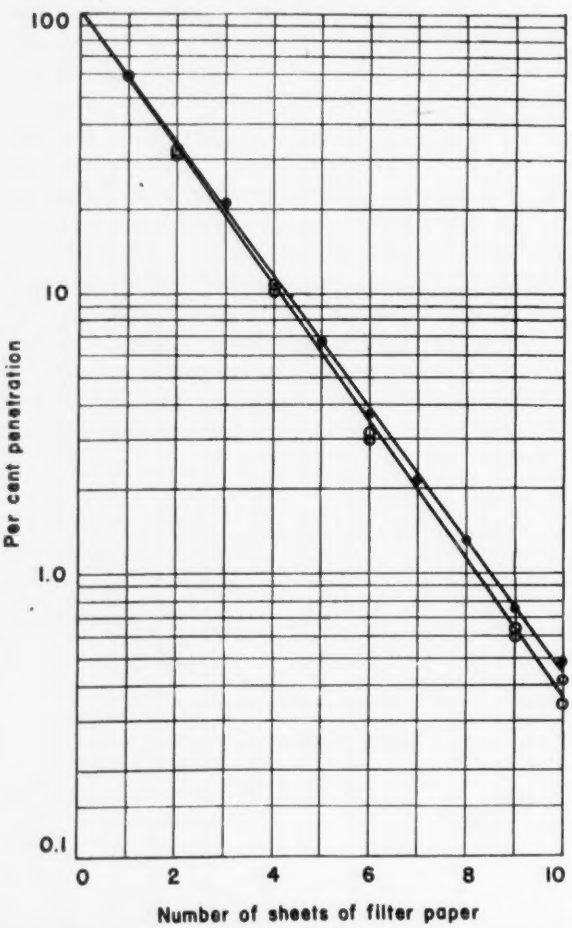


FIG. 9. Validity check of photoelectric penetrometer. (From the *Journal of the American Chemical Society,* **69**, 429, 1947.)

The photocurrent, which is proportional to the light intensity, flows through R and causes a drop in potential, E_R, which is balanced by the potentiometer to obtain a null reading on the galvanometer G in the plate circuit of a single-stage d.-c. amplifier.

The smoke cell is designed to reduce background stray light to a low figure, and the stray-light current is compensated electrically when the cell is filled with carefully filtered air. Next the cell is filled with the raw test smoke, the percent-penetration potentiometer is set on 100, and the sensitivity controls are adjusted to balance the amplifier when the resistance R is 10 megohms. Finally, filtered smoke is passed through the cell and the potentiometer is balanced to read the penetration directly in percentage. For low smoke concentrations R can be increased in decimal steps to 10,000 megohms, so as to keep the photocurrent IR drop within the range of the potentiometer. The instrument is direct-reading at all times, is sensitive to one billionth of a gram per liter of a DOP (dioctyl phthalate) test smoke of 0.3-micron diameter, and can be used to test filters with a sensitivity of one thousandth of a percent.

This photoelectric penetrometer was found to agree with other methods of measuring smoke penetration, in the range where these methods were applicable. At lower concentrations, the self-consistency of a series of measurements with the penetrometer may be checked by means of the Filter law. Figure 9 shows two series of results obtained with one of our instruments. The linearity of the plot of percent penetration, on a logarithmic scale, against number of sheets of filter paper in a composite pad, shows the self-consistency of the measurements over a two hundred and fifty-fold change in concentration. This also indicates a homogeneous test smoke, since the large particles in an inhomogeneous aerosol would be removed chiefly in the first few sheets, and would give a curve concave upwards.

In the summer of 1944 we undertook to develop for the Army Chemical Warfare Service (now Chemical Corps) a supersensitive penetrometer to test the best service canister filters by an automatic count of the individual effluent smoke particles. By June 1945, O'Konski, Pickard, and Gucker had produced a photoelectronic apparatus able to count individual DOP particles of 0.6-micron diameter or larger, at rates from 1 to 1,000 per minute. This apparatus has many other uses in colloidal chemistry, bacteriology, and industry.

The photoelectronic counter is shown diagrammatically in Figure 10. Light from a 50-candle power automobile headlight bulb *A* is focused inside the black-walled cell by a pair of aspheric condensing lenses *L, L,* like those of the penetrometer. A black disk *B* mounted on the glass plate which forms the end of the cell cuts out the central part of the converging cone of light, outlined by dashed lines, so that the smoke particles at *D* are under intense dark-field illumination. The aerosol passes upward at one liter per minute through the central tube T_1, while filtered air flows through T_2 at the same linear rate, to form a protecting sheath which keeps the smoke from circulating before it leaves the cell through tube T_3. Each particle which passes through the focus scatters a pulse of light, chiefly in the forward direction. That portion which is intercepted by the lens *E* is focused upon the photosensitive cell *C*, as indicated by the dotted lines. The stray light, which contributes to the random background noise of the cell, is reduced by means of the blackened baffle *F*, the black disk *B* which prevents reflection from the glass, and the blackened baffle *G*. The electrical pulse from the tube *C* is increased about two hundred thousand times in a pulse amplifier, then fed to a thyratron "trigger" circuit, which actuates a mechanical recorder.

Figure 11 shows the smoke cell mounted on the amplifier chassis. Figure 12 shows the second unit, containing the trigger circuit, timer, and the regulated power supply.

In making a count, recorder 1 or 2 is selected, and the START button is depressed. This starts the count-recorder and timer simultaneously. When the counter has made a complete revolution, it closes a circuit which stops both timer and counter. Thus each experiment gives the time for 100 counts. Depressing the START button automatically resets the timer before it starts the next series of counts. A pair of experiments, using the two counters to check each other, is adequate for a filter test. Since DOP smokes containing as many as 10 billion particles per liter may be used, and

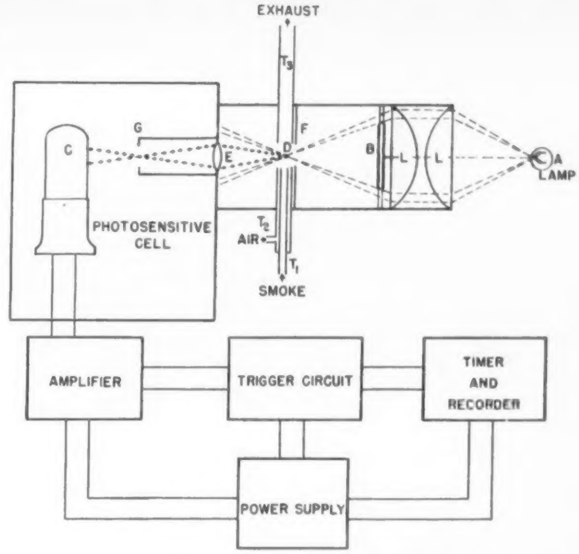


FIG. 10. Schematic diagram of photoelectronic particle counter. (From *Chemical Reviews*, **44**, *373*, 1949, Williams & Wilkins Co., Baltimore, Md.)

as little as 1 particle per liter can be detected, the apparatus is sensitive to one hundred millionth percent penetration.

When longer counts are to be made, the switch is turned from AUTOMATIC to MANUAL, and the auxiliary counter in the upper right-hand corner registers the number of hundreds of counts. The apparatus may be stopped at any even hundred counts by turning back to AUTOMATIC. When the experiment takes less than 100 counts, the counter and timer may be read and the switch in the lower right-hand corner turned from COUNT to RESET. This connects a small relaxation oscillator to the thyratron circuit, and quickly resets the counter to zero.

Tests of the accuracy of the counter are even more difficult than the tests of the photoelectric penetrometer. The most reliable evaluation of the counter has been made by Ronald M. Ferry, Leo E. Farr, Jr., and Mary G. Hartman, who are investigating bacteriological uses of the counter, particularly in connection with the study of airborne infections. Recently they completed a series of experiments in which they compared the counts registered by our instrument with the numbers of aerosol particles determined by means of a carefully constructed two-stage impinger. As test material they used two bacteriological aerosols. The first was made by spraying aqueous suspensions of *Bacillus globigii* (BG), which are ellipsoidal, about 0.8×1 micron in size. The second was an aerosol of *Serratia marcescens* (SM). These are somewhat smaller, sometimes nearly spherical—

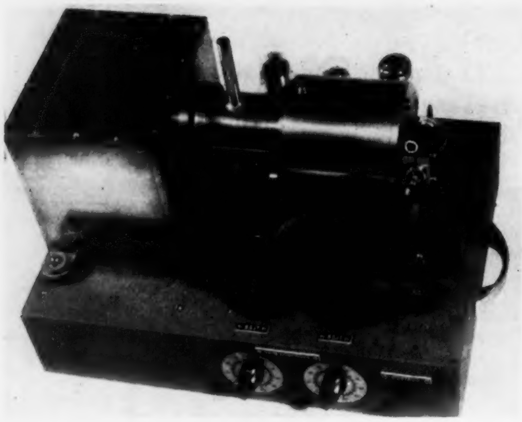


FIG. 11. Smoke cell of particle counter.

and frequently ellipsoidal—in form, and about 0.5×1 micron in size. These investigators found excellent agreement between the results obtained with the two methods. The advantages of the automatic counter are obvious compared to the slow microscope count of the slides in the impinger or with a bacteriological count of viable spores, collected on a cotton filter plug, washed onto an agar plate, incubated overnight, and determined by a count of the resulting colonies. A measurement which requires hours for incubation alone when carried out bacteriologically can be made in a few minutes with the photoelectronic counter.

The original photoelectronic counter was sensitive to individual particles weighing less than one billionth of a milligram. It may have considerable application in bacteriology and in the many industrial processes, like the preparation of penicillin and streptomycin, where large quantities of sterile air have to be used. Bacterial spores and the dust particles in a trace of unfiltered air from the room show up almost immediately in the counter, whereas the bacterial contamination which may be introduced in this way would not ordinarily appear until hours later in bacteriological plate counts. Although the light scattering goes down very rapidly with decreasing particle size, we are attempting to improve the optical system of the counter and thus adapt it to still smaller particles. We are also attempting to adapt the counter to the rapid determination of particle size in aerosols by measuring the size of the electrical pulse given by each particle, which can be correlated with the amount of light scattered by the particle, and hence with its size.

In addition to the optical properties we have just considered, aerosols also show a number of interesting and important electrostatic properties. Frictional electricity has been studied for a great many years. If a rod of hard rubber, glass, or some other insulating material is rubbed with fur or flannel, it will become electrified. The two substances rubbed together will acquire opposite charges, since electrons are transferred from one to the other. Frictional electricity also may be generated when aerosol particles pass through a small nozzle or are blown against a surface. A number of people have observed that wires exposed to a violent snowstorm will be charged sufficiently to emit a continuous stream of sparks or a distinct corona discharge with a current of a number of milliamperes. Very high voltages may be built up in this way during dust storms, and the lightning flashes observed during the eruption of volcanic ashes probably are due to the same cause. Airplanes flying through dust clouds, rain, or snow often become so highly electrified as to give off a blue corona discharge—St. Elmo's fire—which interferes with radio reception. Over a hundred years ago, W. G. Armstrong constructed an apparatus consisting of a boiler from which steam was forced out through a jet, thus forming an electrostatic generator. More recently a number of people have studied the charging of finely powdered minerals and other dielectric materials which were blown through metal tubes. R. E. Vollrath has built an electrostatic generator that can produce a current of 80 microamperes at a potential of over a quarter of a million volts. A jet of air carrying finely divided diatomaceous earth is blown rapidly through copper tubes mounted in an insulated metal sphere. The fine dust is then returned to the original container, which is grounded and provided with a canvas top to allow the air to escape. W. C. Hall has made a study of the possibility of discharging the static electricity from airplanes by means of diatomaceous earth blown out from the plane through brass tubes filled with metal turnings.

Whatever the exact process by which the aerosol

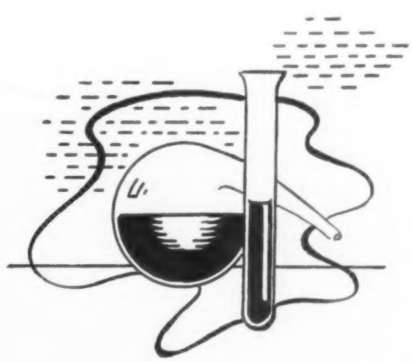
FIG. 12. Time recorder and power supply of particle counter.

particles acquire their static charge, there is evidently a theoretical upper limit to its magnitude. This is reached when the breakdown potential of air is exceeded and the excess charge leaks off. It may be shown that the maximum possible charge on the particle is proportional to the square of its radius—i.e., to its surface area.

An electrostatic particle counter based upon these principles was developed by Arthur C. Guyton at Camp Detrick between June and October 1945. In his apparatus, a stream of air is sucked through a hole 0.8 millimeter in diameter at the end of a 45-degree jet at a very high velocity. The aerosol particles are directed against an insulated metal collector close to the orifice. The solid dielectric particle imparts to the collector an electrical impulse of about 50 microseconds' duration, the voltage of which Guyton concluded was proportional to the square of the particle radius in accordance with the upper limit of charge mentioned above. This pulse is fed through a four-stage amplifier, where it is increased by a factor of one hundred thousand. The output pulse may be viewed on an oscilloscpoe or put through a thyration circuit to operate a mechanical counter. Guyton found that conducting solid particles like iron powder and aqueous drops produced very weak pulses, but that these may be amplified by charging the collector, and that the amplification was in direct proportion to the applied voltage.

In Guyton's apparatus an electrical discriminator could be set to limit the counting to pulses exceeding any predetermined voltage. He determined the size distribution of the particles in an aerosol with his instrument, assuming that the pulses are proportional of the square of the particle diameter. This application requires calibration of the apparatus with particles of known size, and assumes that the pulse voltage depends only upon the size of the particle. Guyton's counter was sensitive only to particles of about 2.5 microns in diameter or larger. Part of this limitation was due to the amplifier circuit which he employed, but it is doubtful if this method could be applied to particles as small as 1 micron in diameter. The initial voltage pulse caused by these small particles is no larger than the voltage fluctuations caused by the random motion of the electrons in the input circuit of the amplifier—the so-called thermal noise of the amplifier. However, the simplicity of the pulse pickup in the electrostatic counter is a decided advantage; it may prove useful in a study of the larger-size aerosol particles and may possibly allow a discrimination between particles of different chemical composition.

The next few years should see an expanded knowledge of the nature and behavior of aerosols, as the new optical and electronic methods are applied to their study. One of the fascinations of these shortest-lived of all colloidal systems is the ingenuity in theory and the experimental skill which their study requires.



THE "NATURAL SCIENCE IDEAL" IN THE SOCIAL SCIENCES

LEWIS WHITE BECK

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THE ORIGIN OF THE "NATURAL SCIENCE IDEAL"

IMAGINE a man who builds a house like the Joneses, at considerable inconvenience to himself because actually he needs something quite different. As soon as he gets the foundations laid, the Joneses begin tearing down parts of their house, adding new wings, and overhauling its foundations. Our poor social climber has committed himself; he has to continue to build according to his plans whether he likes them or not. To comfort himself, he says he has the kind of house the Joneses have and ignores the fact that they are doing a big job of renovating.

This little fable of keeping up with the Joneses fits the relations existing until a short time ago between the social and the natural sciences. The climbers, the social scientists, have tried to imitate the Joneses, the natural scientists. Now the social sciences have an immense house much of which is not very useful; it lacks many of the modern conveniences; but it seems to be scientific, just the same, and that often seems to be enough. But the social scientist might be far happier in his house, or he might be more successful in renovating it to meet modern needs, if he gave up pursuing the past glory of the great edifice of nineteenth-century physics.

When splitting off from philosophy in order to become scientific, the social studies took a bad moment to imitate the natural sciences. They did so just before the natural sciences themselves began to undergo major changes. The result is that many social scientists pride themselves on being natural scientists or regret that they cannot be, whereas the science they emulate or would like to emulate became obsolescent fifty years ago.

In imitating the natural sciences, the social sciences attempted to follow both the methods and the metaphysics of the former. The social studies tried to attend only to observable and measurable entities and to connect these by simple causal or functional laws. If the social scientists thought that they were like the natural scientists in studying "reality," they became mechanists or materialists. If they feared equating their verified hypotheses with "reality," as many natural scientists did, they became positivists. In either case they took over ready-made philosophies of the nature of scientific objects. But there was no unanimity on the philosophical foundations current among the natural scientists, and the "unity of the natural sciences," by virtue of which they might have served as an unequivocal model, was an illusion even before the death of Comte.

The social sciences, therefore, neither emerged from, nor could they later merge into, a homogeneous body of natural science doctrine. The natural science ideal, which many social scientists wished to pursue but which was vehemently rejected by others, was much more ambiguous than it appeared to be in the work of Comte and Spencer. By the time of Dilthey, with his emphasis on the function of sympathetic imagination in social studies, the opposition of the natural and the social sciences was predicated upon an almost complete misunderstanding of the methodological foundations and metaphysical implications of the natural sciences. It would have been much more to the point to have compared the status of the new social sciences with that reached by physics in the time of Galileo than to compare these nascent sciences with a physics already showing signs of passing through the change of life. The contrasts between explanation and description, between nomothetic and ideographic procedures, and between the ideals of a *beschreibende* and a *verstehende* psychology were not so much contrasts between the natural science ideal and the ideals perhaps more germane to the social studies, as they were signs of problems which every science, whether it be natural or social, must face in the early stages of its development.

It is consequently beside the point to contrast the natural and the social sciences in the language used in the early part of this century. Neither the

natural nor the social sciences were homogeneous bodies of doctrine in simple conflict with each other. No clear-cut decision could have been intelligently made between the alternative of following or rejecting the natural science ideal. There were analogous conflicts within both bodies of knowledge between opposing strategies. In each case these conflicts have been resolved in analogous ways during the present century. There is now a continuity of method and philosophy in the two branches of science that could not have been dreamed of even by the most naturalistic of social scientists of the time of Spencer, because this continuity is a consequence of a *rapprochement* in which both sciences have actively participated. We shall see this in detail throughout this essay; at the moment let it suffice to mention the vocabularies of the two. It would not be possible, upon looking into the index of a scientific book, to tell whether it was a book on natural or social science if it contained only the following entries: constitution, dimension, experiment, field, migration, population, prediction, probability, space, statistics, vector. And the list of common terms is growing year by year.

It would be going too far to say that there is no difference between the two groups of sciences, but we should not overemphasize their differences, as was frequently done early in the century, or underestimate them, as has been fashionable since then. It is sound scientific procedure to substitute differences of degree for differences in kind whenever apparent differences in kind can be interpreted as consequences of variation of some common factor. The common variable that I believe will account for both the unmistakable differences and the current *rapprochements* between the natural and the social sciences is "complexity of subject matter." It is my belief that the major differences between them are due to the greater complexity of the subject matter of the social sciences, and that differences of method and interpretation of results are due primarily to awareness of this difference.

If this is correct, we should be able to test it empirically, by seeing whether the social sciences, when they deal with simple subject matters, are able to approach the natural science methods, and whether the natural sciences, when they deal with complex subject matters, appropriate social science methods. Let us, then, turn to an examination of their respective subject matters in order to answer the question: Will differences in complexity account for differences in their observational, experimental, and conceptual techniques?

SUBJECT MATTER OF NATURAL AND SOCIAL SCIENCES

When we think of the social sciences as only the "poor relations" of the natural sciences, we forget that an insight into the order of society was prior to that into nature. Every primitive people sees nature by an analogy with its social organization. Science began when laws, like those given by governments and tribunals, were projected into nature.

The great Greek philosophers approached nature with the anticipation that it would conform to simple principles, some aspects of their society providing them a model for the interpretation of nature. Anaximander (*ca.* 550 B. C.), in an epoch-making analogy, held that changes in nature are regulated by *justice*, anticipating the function later ascribed to *laws*. Henceforth nature was to be seen as a cosmos.

But in searching for regularity and simplicity and lawfulness, the philosophers and early scientists found that they had to work with abstractions from observations and not with complex observations themselves. From the time of Galileo, at the latest, we feel that the "right abstractions" were made, because he chose to report those aspects of his observations which could be related to each other by simple mathematical laws.

Since Galileo, the subject matter of the natural sciences has been relatively simple and repetitive series of simple events. Such series are repetitive because they can be isolated from many other events and understood without reference to them. The natural sciences deal with isolated systems, because the variables they choose to observe are controllable by means of varying other chosen variables. Solely for this reason are simple experiments possible. Only a small number of variables have to be known for us to give functional laws relating one to another.

Certainly every event in nature is related to an untold number of others, perhaps even to every-

thing else in nature. But by abstraction and material isolation, we are able to reduce the effects of most of the others to negligible quantities, and to attend only to the functional relations of certain chosen events. In the natural sciences, lack of repetitiveness in a series of events, as this occurs when an experiment "turns out wrong," is always taken as evidence that the systems were not isolated, and we thereupon carry through a process of successive approximations toward complete isolation.

Nature not only has serial orders which can be studied in relative isolation; the things of nature also come in "vertical" arrangements, or wholes with contemporaneous parts. Field concepts, rather than merely serial concepts, apply to this aspect of nature. Bue because we can isolate systems, we can determine the boundaries of these fields, and eliminate the factors which would make our study of a given whole unmanageably complex. By moving the "isolation partitions," we can determine experimentally the effects of parts on wholes and wholes on parts, even though we never deal with an entity which is not a part of some whole.

The subject matter of the social sciences, on the other hand, consists of highly complex constellations of complex events in systems that are only poorly isolated. Instead of indistinguishable atoms, as the chemist considers his subject matter, the social scientist must deal with societies of individuals of almost infinite internal complexity and variability. No one has yet made the fortunate discovery comparable to that of Galileo in physics: though we know that science cannot deal with an unlimited number of variables, no social scientist has yet shown us precisely which ones to choose to interrelate and which ones may be safely neglected.

When we try to isolate systems in the social sciences, we therefore do not know what to include in them and what to try to eliminate. We cannot move our partition boundaries at will, because the contexts within which we find human beings are not variable to such an extent that we can try out many different wholes for a single part. We cannot isolate a child from all social environments to see where the partition between eliminable and noneliminable environmental factors should be drawn. Until we do so, however, we have no generally acceptable rule by which we can decide what factors to include in our descriptions of the relevant environment or social field. We have parts always within wholes; and, though the social sciences have advanced on the basis of this recognition, which has often in the past not been given sufficient weight, it is hard to specify the relevant part-whole relation because it *always* obtains.

Let us not overlook the fact that these differences are differences of degree, and that as the social sciences approach the stage where they may be able to decide which few variables may be most profitably observed, the natural sciences are undergoing developments of techniques for taking more and more variables into account. It is now recognized that the high regularities of the physical sciences are only statistically simple; as the physical scientist gets closer to the individual object, as it were, the complexities that had been neglected before reappear. Instead of attending only to serial collocations of simple events, the physicist is now finding it necessary, in spite of all his efforts, to deal with field concepts and probabilities as ineluctable parts of his conceptual system. We should not forget that "statistics" is originally a concept and technique of social science, and its use in physical science signifies an often overlooked appropriation of social science methodology.

OBSERVATIONAL TECHNIQUE IN NATURAL AND SOCIAL SCIENCES

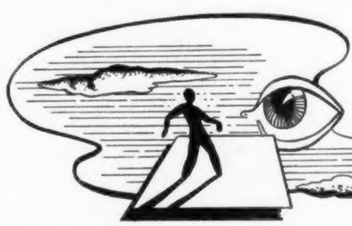
Observation in the natural sciences differs widely from that of everyday life. Most observations in natural science are instrumental results, usually observations of pointer readings. The major part of natural science work is not the taking of observations, but deciding what to observe and constructing instruments to make the observation. The observations of the natural scientist, therefore, are never the raw data or brute facts of common sense; they come to him already conceptually transformed and instrumentally abstracted from irrelevancies. They are what Loewenberg has aptly called "postanalytical data." In getting these postanalytical data, the scientific instrument reduces the subjective contribution of the observer almost to zero and "narrows the field of vision" to a specific observable event uniquely correlated with some unobservable we are interested in measuring.

Until about a century ago, observations in the social sciences hardly differed at all from those of everyday life. The student of social phenomena observed the phenomena of society as a physician would observe a patient if he had no thermometer or laboratory reports. The data of the social studies were "preanalytical." Where the contribution from the object ended and that from the observer began, no one could tell. Because there was no standardized instrument to narrow the field of vision to specific and relevant phenomena, the facts of so-

cial science might vary from common-sense observations to the narrow observations of a man with an *idée fixe*. The facts of the social studies were about as objective as journalistic observation, and no science could be based on such unstable and disputable facts.

As the social studies became scientific, they did so in part by the use of instruments. Usually these were not "brass instruments," but conceptual devices that served comparable purposes—reducing the subjective contribution to observations, and abstracting the desired observable from irrelevant data normally given along with it. But these conceptual devices served the same purpose as physical instruments: they gave an indisputable postanalytical datum which seemed to be uniquely correlated with some vague preanalytic observation or with some wholly unobservable entity in which the scientist was interested.

Consider an intelligence test, perhaps the most nearly perfect of all social science instruments. For a quality not directly observable but the ob-



ject of many common-sense judgments, the test substitutes a postanalytical datum, a ratio between two observed quantities, namely, age and a set of marks on a paper. The set of marks and this ratio are obtained by standardized and conventional procedures. "Intelligence" is not only measured by this instrument; it is operationally defined by the methods used to measure it. Until the test is devised, "intelligence" is not a part of scientific vocabulary at all.

Even with this instrument, the results still differ widely in scientific standing from those obtained with, say, a galvanometer. The galvanometer substitutes a postanalytical datum, a number, for a preanalytical datum, the shock we all feel when we hold a wire under some conditions. The galvanometer standardizes the conditions, eliminates subjective differences between observers, disregards irrelevancies such as the "appearance" of the circuit, and gives us a "hard" and indisputable datum. With a galvanometer, we can forget all about the original shock we felt. But with an intelligence test, we still think that it is measuring something that we already knew about, and if its results conflict too widely with those of our common sense, we decide the instrument must be changed. The social scientist simply does not trust his instruments as much as the natural scientist trusts his. The social scientist rightly reserves some insight against the reduction that his instrument would effect. However much the instruments of social science localize and control the subjective contribution to observations, the design, choice, and evaluation of instruments still depend upon the same kind of insight that social philosophers have always possessed or claimed; otherwise the results of instrumental observation may be very neat and elegant, but they have no noticeable relevance to the prescientific problems which led to the development of these, rather than other, instruments.

Hence the social scientist, equipped with the finest batteries of tests, is still in the position of the legendary people who wished to weigh a pig very accurately. They planed the board to which the pig was to be tied until it was of identical thickness, measured in "milli-micro-mulahs," throughout its length; they used as counterweights stones whose sphericity had been established within limits of one "milli-micro-mulah;" they carefully balanced the pig and board against the stones—then they asked the first stranger who came along to estimate the weight of the stones.

Because the operational definitions of the objects of natural science are applied to terms of no great emotional significance, and are definitions of which there are no counterparts in everyday language, we tend to forget that the way in which the natural scientist has obtained them is logically not unlike that of the social scientist or the legendary pig-weighers. The natural scientist's objects themselves do not determine what aspects shall be observed. The instruments he uses are extensions or projections of the questions he asks. With other questions, there would be other instruments and other data. The choice of his instruments is not ultimately determined by the object, but by the kind of answers he wants. In this respect he is exactly like the social scientist.

But here, again, the natural scientist knows better what he is looking for. As he is interested in correlating his data in simple functional laws, he is interested only in an instrument whose reading will be a variable in an equation by which he can predict what the reading on another instrument will be. He uses only those types of instruments which will give him such results; even further, he uses only those *specific* instruments which will give him those results, and sends the others to the shop. The social scientist, however, is lucky

if he possesses even a single instrument for getting data. Outside a few fields, such as that of factor analysis in psychometrics, he must correlate his instrumental results with his vague common-sense preanalytical observations; he therefore has little or no check on the accuracy of his instruments. In consequence, although the introduction of instruments into the armory of social sciences has given intersubjectively valid data which the social scientist did not formerly have, it has not permitted him to state categorically what is the conceptual significance of his results. He must still "estimate the weight of the stones."

Hence observation in both the natural and the social sciences necessarily involves a subjective element of choice of observable variables. But, whereas in the natural sciences this choice is constantly modifiable by reference to other chosen observations, in the social sciences the choice is usually corrigible only by reference to the "enlightened common sense" of the observer, which tells one social scientist (but unfortunately often him alone) what weight is to be attached to the results, which observations are worth getting, and which ones are not. We can see the reason for this in the differences in complexity of the sciences: the instrumental "sieving" of the facts of nature is very precise and fine-grained, whereas the facts of society are large-grained and recalcitrant to narrow abstractive procedures, whether instrumental or conceptual.

EXPERIMENTAL TECHNIQUES IN THE NATURAL AND SOCIAL SCIENCES

Often the contrast between the natural and the social sciences is as succinctly drawn as that between experimental and observational sciences. But there are nonexperimental natural sciences, and there are experiments in the social sciences. This contrast, therefore, is not perfect; but it throws light on another consequence of the different complexity of the two kinds of science.

The natural sciences, as we have said, can establish physically isolated systems in which only a small number of variables play a significant role; therefore, an experimental determination of their correlation is possible. The social sciences cannot physically or temporally isolate their subjects. Though experiments may be performed under conditions of imperfect isolation, neither in physics nor in sociology would we know how much of the object we were experimenting with. The physicist can meet this objection by moving his partition boundaries; the sociologist cannot. An experiment on children puts the boundary, let us say, at 9:00 A.M. in a classroom; but the previous history of the child, the home conditions, the hereditary conditions, and so on are uncontrolled variables from which the subjects of the experiment are by no means isolated. The social scientist, therefore, has to perform the same experiment over and over again with the idea that the uncontrolled variables will be randomly distributed in the series and thus cancel each other out. Hence in the experiments of social science there is a large inductive element lacking in the interpretation of good experiments in the natural sciences.

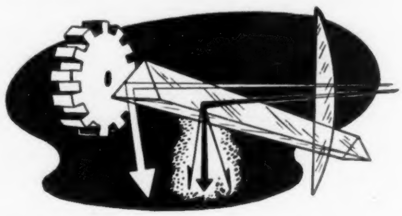
In recent years the social scientists, especially Lewin, have developed techniques for deriving results from only one or a very small number of experimental situations. This is possible when there are a large number of variables within the "field," so that some interconnection between them can be found and little or no recourse has to be made to relatively unknown variables outside the field. Work of this kind, in which the conceptual apparatus is adequate to the complexity of the subject matter, is one of the most encouraging signs of a further affiliation between the natural and the social science techniques. In contrast, when the external trappings of a natural science experiment are imitated, so that only a few highly abstract data are obtained, the lack of isolation of the variables being measured really prevents the experiment from being comparable to those of the natural sciences.

There is another difference between the experiments of the two branches of science which is dependent upon differences in their complexity. Isolation from the operator is difficult to achieve in the social sciences; the adventitious circumstances of the experimental setup, the isolation partitions themselves, function as significant causal variables. From the experimental results we can extrapolate to "normal situations" only with a wide margin of error, since these variables may be very important in the experiment and wholly absent in the situation we wish to make predictions about.

In experiments in natural sciences, the experimental situation is comparable to the normal, or at least the effects of the experimental situation can usually be estimated and conceptually eliminated. Certainly putting a new meter into a functioning circuit affects the circuit as a whole, but this effect can be measured in other experiments on the meter itself and we can eliminate the interference.

As the physical sciences come to deal more and more with the "individual physical object"—e.g.,

a single particle—it is found that the experimental conditions may play a more disturbing role which cannot be eliminated. The Heisenberg principle of uncertainty is illustrative of this comparatively unusual situation in the natural sciences, but one very common in the social. The study of individual members of a population of electrons may suffer from many of the same disabilities as the



study of human individuals in society. As physics turns its attention to the complexities of the individual case, and sociology finds itself able to deal with large numbers of cases, their operational conditions and results become more nearly comparable.

Each science begins with "middle-sized" facts, those which are within range of convenient observation. The middle-sized facts of physics have a specious simplicity because individual differences have been statistically canceled out; if physics, like the human sciences, had begun with the individual case, it is likely that it would have made no more rapid progress than sociology. The middle-sized fact of sociology is the small community, and this is more complex and variable than the individual particle in physics. As sociology approaches statistically evened-out states of affairs, it may approach the simplicity of classical physics, which dealt only with its evened-out, middle-sized facts.

THEORETICAL STRUCTURE OF THE NATURAL AND SOCIAL SCIENCES

The differences between the theoretical structures of the natural and the social sciences are even more obviously contingent upon differences of complexity in their subject matter. We shall see this in two respects: the parsimony of the two systems, and the modes of explanation in the two systems.

First, a word about the theoretical structure of any science. In scientific research there are three types of hypotheses functioning. First, there is the *substantive* hypothesis, the hypothesis being tested. Second, there is an *operational* hypothesis, stating that if such and such things are done, such and such observable results should be attained, provided the substantive hypothesis is true. The operational hypothesis is always formulated as a basis for experiment or observation, and it is chosen in the light of the substantive hypothesis we wish to test. Finally, there are *collateral* hypotheses, which are not being tested at the moment, but which provide the route by which the mind moves from the substantive to the choice of the operational hypothesis.

To illustrate these hypotheses, let us take an exceedingly simple example. We have the substantive hypothesis "Salt is soluble in water." We test it by performing an experiment based on an operational hypothesis: "If I put the crystals from this bottle into water, they will disappear." How do we move from the former to the latter hypothesis, by which it is to be tested? We do so by means of certain collateral hypotheses, viz., "These crystals are salt," "This liquid is water."

A given hypothesis is not inherently substantive while another is always collateral. We may subject any one of them to test. In our previous example, for instance, we could test the hypothesis "These crystals are salt," using the other hypothesis, "Salt is soluble in water," as collateral.

When an observational result differs from the prediction from a set of hypotheses, it is always possible to choose whether we shall consider (*a*) the operational hypothesis to have failed (experimental error); (*b*) the substantive hypothesis, the one we intended to test, to be wrong; or (*c*) some collateral hypothesis, by virtue of which we choose this experiment, to be in error (systematic error).

If we decide on the first alternative, we are in effect "testing a fact by a theory." This is sometimes necessary in even the best-organized sciences in order to avoid renegade instances and to give credit to the obvious fact that not all observations are equally trustworthy. But science becomes dogmatic if this procedure is always followed, because then there can be no occasion to modify a theory once adopted. We have already seen the difficulty of eliminating experimental error in the social sciences, and consequently in them frequent recourse is had to this expedient; if the result is not as predicted, we can always say that there were disturbing and uncontrolled factors, or the observer was inaccurate, or the like.

Assuming that the experiment has been done well, we then have a choice as to which of the other hypotheses is to be modified or rejected. In the natural sciences this choice can be made by performing still other experiments involving different collateral hypotheses (in our example, we could use crystals from another bottle), or by undertaking other experiments in which the collateral hypothesis is tested without reference to the hy-

pothesis in which we were originally interested. (In our example, we could undertake a chemical analysis of the crystals in the bottle to see if they are sodium chloride.) The result of this multiplicity of approach is that in the natural sciences there need be no untestable hypotheses, and every well-performed experiment is crucial for *some* hypothesis in the body of the science.

Because of the complexity of each hypothesis in the social sciences, testing seriatim is rarely possible. For instance, we wish to determine the existence or nonexistence of racial differences in intelligence. We give a test to a group of children of different races. Their marks differ significantly. Does that prove the hypothesis that there are significant differences? Not unless we assume the collateral hypothesis, namely, that the test is independent of cultural differences. Can we test that experimentally? Only by devising a test in which the different cultural groups make approximately the same marks. But usually we cannot independently control the racial and cultural components; therefore, we do not know which hypothesis—a hypothesis about our particular intelligence test, or a hypothesis about the intelligence of different races—must be rejected.

Because some assumptions are untested in our experiment, there will be disagreement about them. The result is that we have "schools" of psychology and sociology (e.g., "racial theories" and "cultural theories") that are distinguished by disagreement about collateral hypotheses which function as "presuppositions." Crucial experiments which might resolve controversies between schools are thus almost unknown in the social sciences. The route by which we move from a substantive hypothesis to an observation or experiment is so circuitous, and involves so many assumptions, that experiments can usually be cited equally well by both sides in a controversy.

The hypotheses of the natural sciences are so simple that they can be tested seriatim; those of the social sciences are so complex and interpenetrating that we have to take them in families. Nevertheless, here again the difference is one of degree, and recent science is narrowing the distance between the two theoretical structures. The natural scientist now realizes that no hypothesis can be tested without assuming others, and ultimately a circle in testing is completed. There now exist in physics several alternative families of hypotheses in which the circle has been completed. All the observations of one are translatable into results of the other, though the two sets are not logically equivalent, and future observations *may* lead to decision between them. At present, the choice must be made in terms of their relative parsimony. Yet the estimation of the degree of parsimony involves aesthetic, procedural, and subjective considerations of elegance, ease of inference, and the like. The philosophy of science during this century has largely emphasized subjective elements in even the most objective sciences, and we find a prominent physicist speaking of science as "nature refracted through human nature." If the subject matter of physics were as complex as that of the social sciences, this human refractivity and selectivity would be more obvious than it is. If the subject matter of physics permitted the same variety of abstractions to be parsimoniously organized, it is likely that the conceptual structure of the natural sciences might appear as arbitrary as that of sociology or political science.

Finally, we come to the general strategy of explanation in the two branches of science. I do not refer to the age-old problem of mechanical *vs.* teleological explanation, for this metaphysical controversy appears in both types of science. I refer rather to the logic of explanation. In the natural sciences, the chief mode of explanation is description of the more pervasive and abstract features of the situation, whereby prima-facie different states of affairs are described in the same terms. For instance, a freely falling body and the moon are special cases falling under Newton's laws. Explanation in the natural sciences is therefore analytic or reductive, through discovery of common and simple conditions of diverse effects whose prima-facie description would involve a very large vocabulary. Hence a phenomenon in chemistry is explained when it is described in the simpler terms of physics; the motions of the planets and of bodies rolling down an inclined plane are explained when a common set of variables is discovered in the description of each phenomenon.

Certainly this relation between explanation and description is met with also in the social sciences. We would, for instance, describe war and migration in quite diverse terms; but we might explain them in terms of a condition not obvious in either but underlying both, e.g., "population pressure." We shall, in the following section, deal with the limits of this type of explanation as one of the unsolved problems in the logic of the social sciences. Still, it must be admitted that at least at present the common mode of explanation in the social sciences is not reductive and analytic, but synthetic. That is, we predict some event in terms of psychology alone; but for more complex events we have to add to the psychological causes suffi-

cient factors to get to the effect we actually find. Thus we say that we must attend not only to the psychological conditions, but draw in also the sociological, the economic, and the like. What would be called explanation in the natural sciences is all too often seen as "oversimplification" in the social sciences.

The extent to which reductive techniques should be universally employed, especially in psychology in its relation to physiology, is one of the crucial problems in the philosophy of social science. Just as physiological description is translated into physiological explanation, it is often held that the logically simpler is everywhere the explanation of the more complex, and psychology must be "reduced" to physiology. If this is the case, then of course there is no autonomous social science; it is simply a division of labor to be tolerated only until the natural sciences are able to effect a reduction. Such reducibility, if it exists, strengthens the thesis that the difference between the two branches of science lies in their differing complexity. The argument of the reductionist is that in the future the natural sciences will become better able to deal with states of affairs of high complexity, and the social sciences will have succeeded in conceptually diminishing the complexity of societal facts, so that the transition can be made. At present it cannot be done, perhaps because of the great disparity in degrees of complexity. Whether it can and should be done is one of the unsolved problems to which we now turn.

SOME UNRESOLVED QUESTIONS OF SOCIAL SCIENCE STRATEGY

There are two problems we have lightly touched on, but which deserve more than passing notice even in a brief discussion. The first is strictly metaphysical: Are there any indigenous and irreducible categories of societal nature (e.g., culture, personality) that will successfully resist all attempts at reduction? Is the *only* difference between societal and natural reality a difference in complexity?

I have called this question metaphysical rather than scientific, for, whatever answer we give to it, the effect on scientific procedure will be the same. Different answers to this question will affect only the philosophical evaluation of the findings and procedures of the social sciences. Admitting irreducible categories would not in the least exempt the social scientist from reducing all that he can in order to increase the likelihood that the remaining ones *are* irreducible and not simply nonreduced. He would still do everything in his power to diminish the scope and importance of the not-yet-reduced concepts. Following the principle of parsimony, he would and should try to account for as much as possible by means of reductive explanation.

Analogous questions are met with in the natural sciences. The world of nature is not prima facie homogeneous, but has manifold discontinuities, levels of organization, and emergent properties. Acceptance of these with "natural piety" would have arrested the development of science. Yet the natural scientist does not have to explain them away in order to be scientific; he has only to attempt to explain them by showing the conditions under which they occur. Inasmuch as the necessary, but not the sufficient, conditions of life have been found in chemical studies, there still remains a task for the biologist—the description of his own phenomena, and the interrelation of them under

unreduced biological categories. There can thus be purely biological explanation if he is successful in elaborating a general system of biological categories.

Similarly, in the social sciences it may be argued that, though it is important to know the natural conditions of societal phenomena, the social sciences have an indigenous subject matter, their own categories for its elaboration (e.g., "meaning" or "value"), and their own techniques for dealing with them (e.g., "understanding"). Much can be said for this point of view so long as it is not allowed to arrest the reductive procedures by which societal phenomena can be related to those of nonhuman nature. We have to deal here with two basic principles of method.

In the logic of science there is a principle as important as that of parsimony: it is that of sufficient reason. The former directs us to look for simplest causes; the latter cautions us not to simplify so far that the explanation is inadequate to the facts to be explained. Opposition to the hegemony of reductionism, insisting on the autonomy of social science categories, emphasizes the importance of the maxim that the adduced reasons shall be sufficient, rather than that they shall be parsimonious. Parsimony is not itself a simple criterion of a good methodology; we cannot simply count the factors of explanation and say that the theory containing

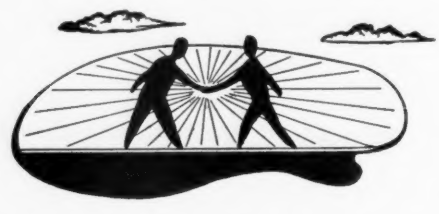
the smallest number is the best. The ideal of parsimony cannot be expressed without the proviso that the conditions for which it is a norm shall themselves be adequate. But if simplicity is difficult to define adequately, how far from simple it is to define adequacy!

Whether an explanatory system is adequate depends in the final analysis on what we want of an explanation. No one holds a brief for an "autonomous chemistry" and for the "indigenous and irreducible facts of chemistry" and thus fights physicalistic reduction. But for practical, even if not for metaphysical, reasons, a comparable reduction of social science concepts to those of natural science may be quite legitimately resisted. Even if we overlook the possibility of metaphysical discontinuities between nature and man, the social sciences, if they are to be of use either practically or for the sake of an insight into social problems, are inextricably tied to enlightened common sense with its terminology. Operational simplification of sociological terms may be oversimplification in the sense that the problems as solved in the reduced vocabulary of natural science are not practically or intuitively equivalent or germane to the problems that originally led to the undertaking of the study. It may be that we ask for the bread of social insight and are given stones of natural science. It may be that an explanation of societal events in natural terms will have to be translated back into original language before anyone will admit that the explanation is adequate to the problem at hand.

Translations of this kind seem trivial and inadequate to the understanding of the initial problem. At the present stage of social sciences, then, it is quite defensible to hold that the explanatory concepts shall be germane to the motivating problem and not simply statements of correlations between societal and nonsocietal phenomena. This being the case, the tasks of the social sciences are the determination of adequate germane categories, such as culture, meaning, function, and value; their rigorous definition within the context of social science phenomena; their theoretical elaboration into parsimonious explanatory systems; and the establishment of rigorous procedural rules for their empirical application. For these tasks, the history of the more highly developed sciences may provide useful cues, but no more.

The second problem, though closely related to the former one, is logically independent of any answer we give to that question. Assuming that reduction will be practiced as far as possible, we still have to decide the proper procedures with respect to the concepts and hypotheses which have not yet been reduced. It is a question of the strategy of theorizing in the social sciences themselves. To be more specific: The social scientists now debate the question as to whether the chief desideratum is a general overarching theory or a series of particularistic hypotheses of relatively low degrees of generality.

The history of the natural sciences provides a valuable guide to the answer to this strategic problem.



The physicists kept their hypotheses as close to observations as possible; their theories were integrations of hypotheses, not highly abstract summaries of concrete facts all on the same level. Galileo and Kepler had to do their theoretical as well as observational work before there could be Newton's. But, we may be told, the social scientists now have almost as many hypotheses as facts; more unkindly, they may be said to be long on hypotheses and short on facts.

The mind of man, however, is not so prodigal of imaginative hypotheses that it can generate an infinite variety of them. Hypotheses show an inner kinship of common parentage in a given milieu and in the inventiveness of the social scientists—the demonstration of this being one of the great accomplishments of the sociology of knowledge. Hypotheses are increasing in number, but the variety of their types may be diminishing. General theory is not to be built by addition of hypotheses, except indirectly; it arises from their analysis and reduction.

Hence it may be expected that when a plethora of facts is elaborated in hypotheses of low generality, the broad outlines of an overarching theory may be subtly adumbrated. But in view of the complexity of subject matter, the looseness of theoretical structure, and the uncontrolled character of many of the observations of society, it is too soon to expect—indeed, it is too soon to be impatient for—a Newton of the social sciences.

EXPLORING THE OZONOSPHERE

CHARLES JAMES BRASEFIELD

Dr. Brasefield (Ph.D., Princeton, 1927) has been Physicist, Signal Corps Engineering Laboratories, since 1941. He is the author of numerous articles dealing with research in positive ion analysis, high-frequency discharges in gases, ionization of gases by positive ions and atoms, nuclear physics, crystal luminescence, and meteorological measurements.

RESEARCH in upper-atmospheric physics has been stimulated in recent years both by a growing appreciation of the importance of physical phenomena at high altitudes and by the increasing availability of equipment suitable for investigating these phenomena. The most fertile part of the unexplored upper atmosphere is the region between 100,000 feet and 150,000 feet in altitude. Since this is the region of maximum ozone concentration, it may be called the ozonosphere. During the past ten years, considerable information on the temperature structure and, to a lesser extent, on the humidity and wind structure of the first 100,000 feet of the atmosphere has been collected by means of the radiosonde. This instrument is essentially a radio transmitter, whose signals vary depending on the pressure, temperature, and humidity of the air through which the radiosonde is rising.These signals are received and recorded by a ground station. By evaluating the ground station record, it is possible to determine the altitude of the radiosonde at successive points in its flight and the temperature and humidity of the atmosphere at these altitudes. Furthermore, if the transmitter is tracked by a directional receiving antenna, it is possible to compute the magnitude and direction of the winds encountered by the radiosonde throughout its flight. The balloon used to carry the radiosonde aloft is called a sounding balloon. The largest sounding balloon heretofore developed by the Signal Corps Engineering Laboratories and supplied to the U. S. Air Force for field use weighs about five pounds and has a diameter of about five feet when barely inflated. Balloons of this type generally burst around 100,000 feet, although occasionally one will reach an appreciably higher altitude.* A search of the technical literature indicates that the previous record for high-altitude balloon flights may have been made in Russia, where a balloon was reported to have reached 131,000 feet.[1]

The ozonosphere is a relatively unexplored region of the atmosphere. Our knowledge of the physical properties of this region has been obtained principally from indirect experiments rather than from soundings made in the region. It is known that ozone strongly absorbs radiation in the far ultraviolet (3200A–2200A). Measurements of the intensity of the solar spectrum in the far ultraviolet prove that ozone in the atmosphere is responsible for the effective termination of solar radiation at 2900A, and indicate that the concentration of ozone is a maximum (7 parts ozone to one million of air by volume) at about 130,000 feet. Absorption of ultraviolet radiation by ozone causes the temperature of the ozonosphere to increase with increasing altitude. This absorption is so strong that most of the ultraviolet radiation is absorbed by traces of ozone at approximately 200,000 feet, and consequently the temperature of the atmosphere should reach a maximum at about this altitude. This has been confirmed by experiments on anomalous sound propagation. Haurwitz[2] and others have suggested that the missing link between solar activity and surface weather may be found in the ozonosphere. They suggested that the larger quantities of ultraviolet radiation presumably emitted by a solar flare may cause disturbances in the ozonosphere that would be reflected in pressure changes at the earth's surface. If this reasoning is correct, then it is obvious that measurements of temperature, ozone concentration, and possibly water-vapor concentration, obtained by balloon-borne soundings in the ozonosphere, should be invaluable for weather forecasting.†

* On September 15, 1948, the Associated Press reported that a sounding balloon released at the USAF weather station at White Sands, New Mexico, reached a height of 120,000 feet.

† It is possible that there are localized regions in the ozonosphere which are especially sensitive to solar flares, and that these regions could be detected if the temperature structure of the ozonosphere were known. The projection of these regions on the earth's surface would then identify the areas in which disturbances in the general circulation may occur at the time of a solar flare.

FIG. 1. An ozonosphere balloon just prior to release.

Information not only on the temperature structure of the ozonosphere but also on the wind structure may prove to be an important aid to weather forecasting. For a knowledge of the winds at these altitudes should contribute significantly to our understanding of the general circulation of the atmosphere.[3] Furthermore, data on winds and temperatures in the ozonosphere may be useful in designing and launching rockets and in evaluating the performance of these rockets.

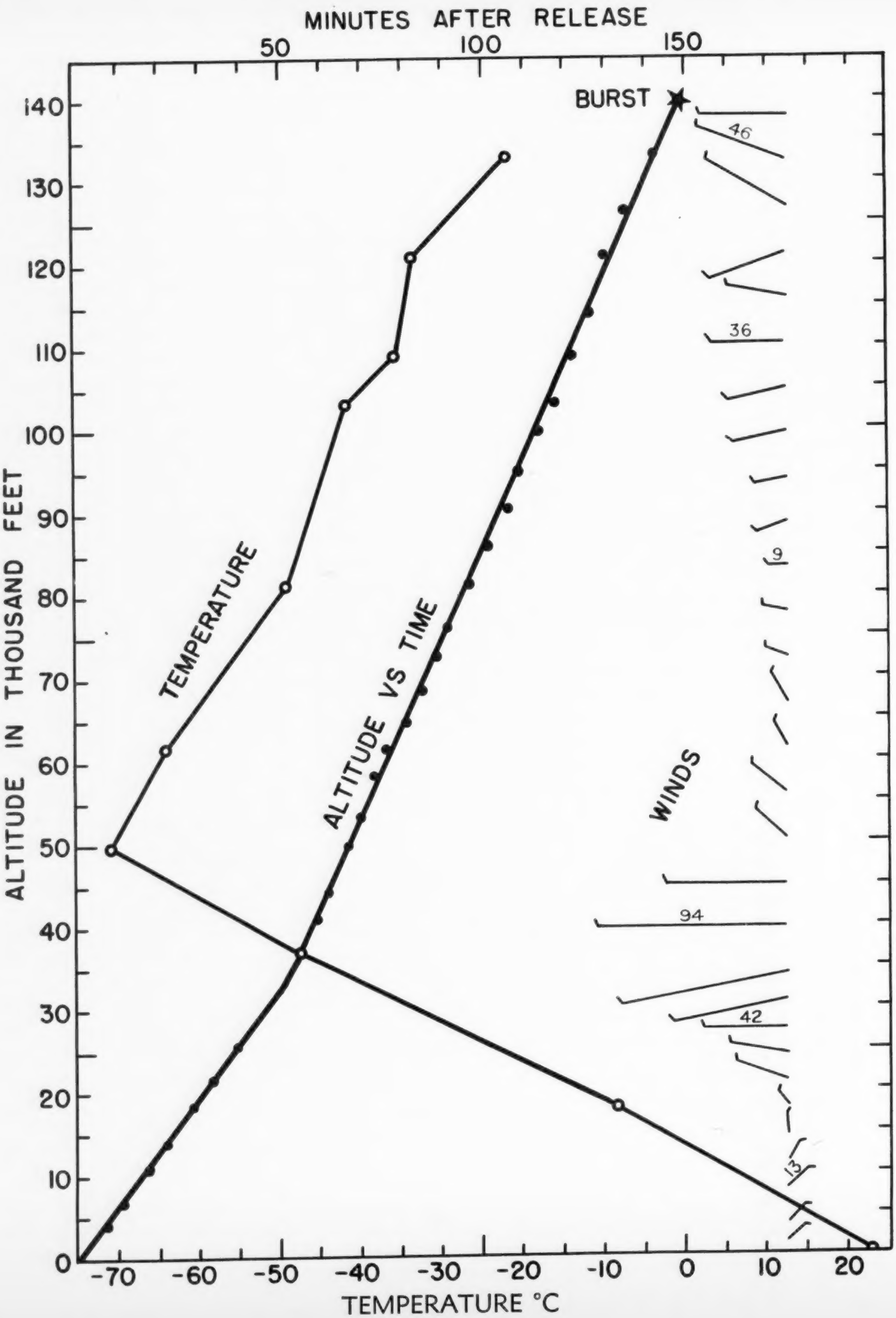
There are several problems involved in extending the limit of radiosonde flights from 100,000 to 150,000 feet. Perhaps the greatest of these problems is that of obtaining a suitable balloon. As has already been mentioned, sounding balloons heretofore available are capable of carrying a five-pound radiosonde to approximately 100,000 feet. Even if these balloons carried no pay load whatever, they would burst at approximately 115,000 feet; consequently, there would be little gain in maximum altitude if a cluster of two or more balloons were used to carry the radiosonde. What is required is a much larger balloon; how much larger can be easily estimated. The pressure at 100,000 feet is approximately 10 millibars (at the earth's surface, normal pressure is 1,013 millibars); at 150,000 feet the pressure has decreased to about 1 millibar. Thus, a balloon that will support a radiosonde at 150,000 feet must have ten times the volume required at 100,000 feet if no increase in weight of balloon is considered. Assuming that the weight of the balloon must be increased by a factor of five, the required volume at 150,000 feet must be about thirty times the bursting volume of the five-pound sounding balloon. This means that the surface area of the required balloon must be ten times greater than the area of the five-pound sounding balloon. In the initial design of the ozonosphere balloon, it was planned to gain a tenfold increase in surface area over that of the five-pound sounding balloon by increasing the weight by a factor of five and decreasing the film thickness by a factor of two. This objective was not quite attained in the first balloons to be developed,‡ which weighed about 17 pounds and whose barely inflated diameter was about 16 feet. When used to carry a radiosonde aloft, this balloon is not completely inflated when it leaves the earth's surface (Fig. 1). It first becomes spherical at about 35,000 feet, and when it reaches 140,000 feet its diameter is about 75 feet.

The accuracy with which the altitude attained by the balloon may be computed depends primarily on the accuracy of pressure measurements throughout the flight. The probable error of pressure measurements made with the usual radiosonde is in the neighborhood of 2 millibars. This error is inconsequential at 50,000 feet, where the pressure is about 100 millibars, but becomes serious at 100,000 feet (10 mb), and is intolerable at 150,000 feet (1 mb). It was therefore necessary to develop a more accurate pressure-measuring element for the radiosonde.§ The objective of this development was a pressure element which, at low pressures, would be accurate to 10 percent of the measured pressure. This would permit altitudes to be computed with an accuracy of about 2,500 feet. It should be noted that this objective requires that in the vicinity of 140,000 feet pressure be measured with an accuracy of 0.2 mb, and in the vicinity of 150,000 feet with an accuracy of 0.1 mb. These are very exacting requirements for flight equipment. At the present time, insufficient test data are available to determine the probable error of the new pressure element, but it is estimated to be in the neighborhood of 0.2 mb in the range 1–2 mb. (As an additional check on the accuracy of the new pressure element, future high-altitude radiosonde flights will carry as accessory equipment a small hypsometer, which should measure small pressures with an accuracy of 0.1 mb.)

During the summer and fall of 1948, about twenty radiosonde flights were made from the Evans Signal Laboratory at Belmar, New Jersey, using development samples of the ozonosphere balloon. The performance of these balloons varied, owing to changes in manufacturing techniques, but on the average the balloons reached an altitude of about 120,000 feet. Four balloons went above 130,00 feet, and at least one balloon reached 140,000 feet, which is a new record for sounding balloons. The results obtained from this flight are shown in Figure 2. It can be seen that the balloon rose about 650 feet per minute until it reached 35,000 feet, after which the rate of ascent increased

‡ This development was undertaken by Molded Latex Products, Inc., of Paterson, New Jersey, under contract with the Signal Corps Engineering Laboratories. The manufacture of the ozonosphere balloon requires equipment of a size unheard-of in the balloon industry. A spherical shell of gelled neoprene latex 57.5 inches in diameter is inflated to a diameter of 25 feet. After drying and curing, the balloon shrinks to a diameter of about 16 feet. Recently the equipment has been enlarged, and now spherical gels 81 inches in diameter are being inflated to a diameter of 30 feet. The first samples of the larger balloon, delivered in December 1948, weigh about 25 pounds and have a barely inflated diameter of about 19 feet.

§ This development was undertaken by Washington Institute of Technology, of College Park, Maryland, in accordance with design and performance specifications prepared by the Meteorological Branch of the Signal Corps Engineering Laboratories.


MINUTES AFTER RELEASE
50
100
150
BURST
46
36
9
94
42
13
TEMPERATURE
ALTITUDE VS TIME
WINDS
ALTITUDE IN THOUSAND FEET
0
10
20
30
40
50
60
70
80
90
100
110
120
130
140
-70
-60
-50
-40
-30
-20
-10
0
10
20
TEMPERATURE °C

to 1,080 feet per minute. The temperature of the air, which was 23° C on the ground, fell to a minimum of – 71° C at 50,000 feet, then rose to – 22° C at 133,000 feet. The winds aloft were generally westerly, with a maximum speed of 94 miles per hour at 40,000 feet. On previous flights during the summer, the winds were also westerly below 50,000 feet, but were predominantly easterly above 60,000 feet, so that the distance of the balloon from the launching site was decreasing during the latter part of the flight. On several occasions under these conditions, the balloon was easily visible to the naked eye at an altitude of 120,000 feet.

Plans for the future include daily flights to 150,000 feet, in an attempt to correlate the temperature of the ozonosphere with surface weather. Measurements will also be made of the diurnal variations in the temperature of the ozonosphere. In addition, simultaneous flights will be made from widely scattered points in order to obtain a better knowledge both of the temperature structure of the ozonosphere and of the atmospheric circulation in the ozonosphere. These data will be correlated with similar data obtained in another phase of the upper-atmosphere research program of the Signal Corps Engineering Laboratories, namely, the measurement of temperature, winds, and gas composition at high altitudes by use of rockets.[4]

FIG. 2. Results obtained from balloon flight at Evans Signal Laboratory, Belmar, New Jersey, on September 28, 1948, 1:20 P. M. The length of the wind vector is proportional to the wind speed (mi/hr). The tail of the wind vector indicates the direction from which the wind was blowing. Thus, near the ground, the wind was northeasterly; at 15,000 feet, northerly; at 40,000 feet, westerly; and at 50,000 feet, northwesterly.

These experiments have required the cooperative effort of several sections of the Meteorological Branch of Evans Signal Laboratory. Thanks are due especially to Lt. Col. A. F. Cassevant, Director of the Laboratory, for his generous support of this program; to Dr. Michael Ference, Jr., Chief Scientist, for his enthusiastic sponsorship of upper-atmospheric research; to Mr. A. Arnold for valuable advice in the design of the balloon and for supervising the launching of balloons; to Mr. W. C. Conover for assistance in calibrating radiosondes and in evaluating flight records; and to the Rawin Section for supplying high-frequency transmitters and for operating the direction-finding equipment.

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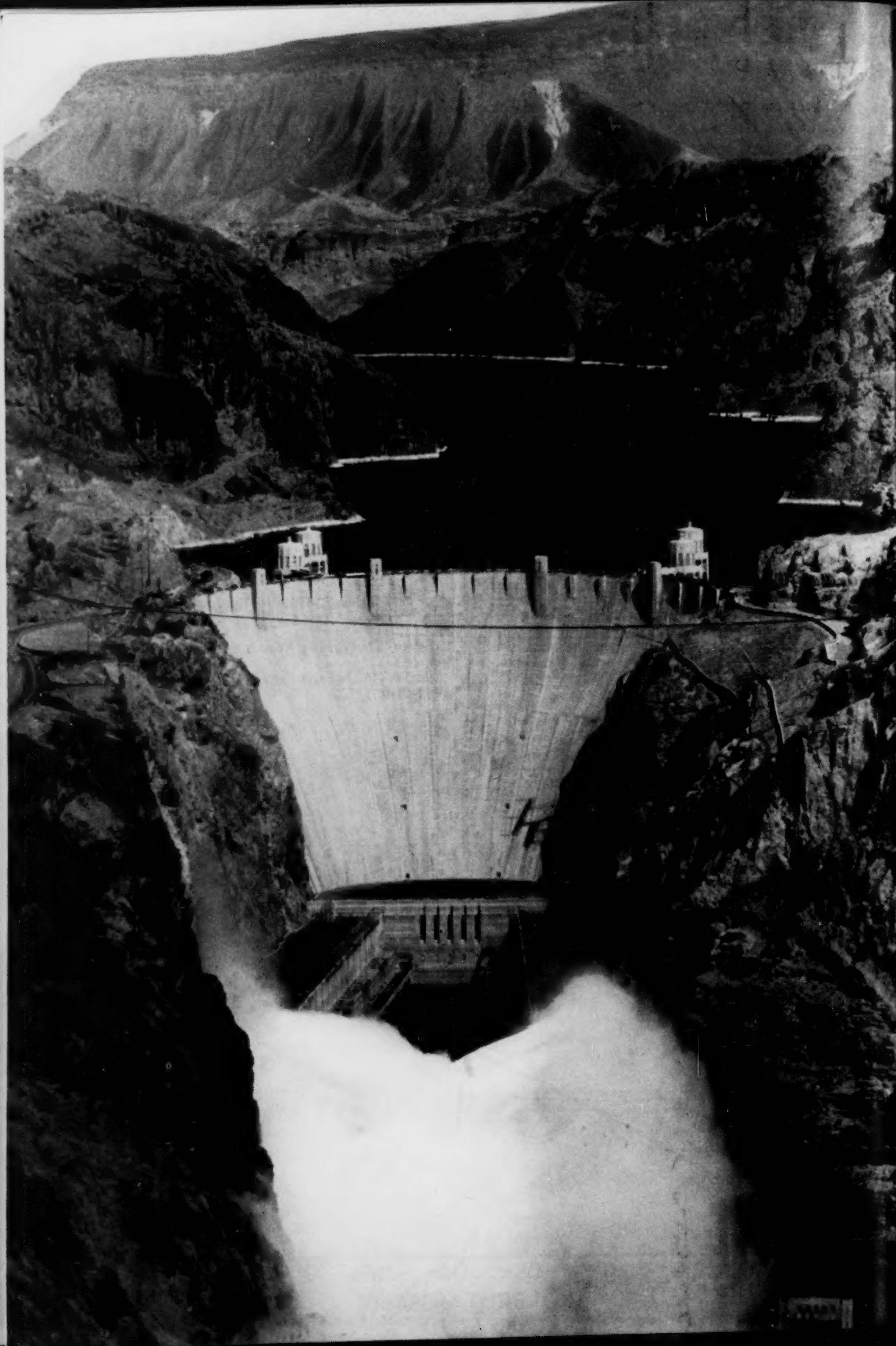


WATER, WATER, EVERYWHERE, BUT . . .

BERNARD FRANK and ANTHONY NETBOY

Mr. Frank (M. F., Cornell, 1929), assistant to the chief, Division of Forest Influences, U. S. Forest Service, has been engaged in watershed management research for the past eleven years. In collaboration with Mr. Netboy (M.A., Columbia, 1928), former editor of the U. S. Forest Service, he is writing a book—of which this article will be the first chapter—to be published next year by Alfred A. Knopf, Inc., under the title "Water, Land, and People."

WATER—especially in the humid regions of the United States—is commonly regarded as ubiquitous and plentiful. Produced in a diversity of ways, it is an indispensable component of all forms of life, from the elephant to the microbe, from the giant sequoias to the tough, crusty lichens clinging to the sides of a granite cliff.

Like other natural resources, such as forests and minerals, water is unequally distributed over the earth's surface. Even in regions blessed with abundant rainfall, the supply is definitely limited. Not all the rain or snow that falls on the earth remains on the surface. Some evaporates into the air, sinks into deep crevices or fissures in the underlying rock—where it may be out of reach of wells and pumps—or returns through subterranean channels to the oceans. The balance is potentially useful water, and its volume and quality are determined, to a significant extent, by factors generally within man's control. Regions sorely deficient in water remain barren or uninhabited, or at most capable of supporting only primitive forms of human society.

Useful water is not "free" or excessively abundant anywhere in the United States. It is largely a commodity whose production and distribution require the outlay of labor and materials and often the application of unusual skill to control its flow and deliver the right amounts and desired quality to the right places. This is true of drinking water, irrigation water, and water for hydroelectric power, inland navigation, manufacturing, and even for fishing and recreational purposes. Safe water "in the raw" is today available on a relatively small portion of the nation's 3 million square miles, mostly in the high plateaus and mountain ranges not yet penetrated by highways, railroads, settlements, towns, or industries. Only in such relatively inaccessible regions, where the water has been filtered and cleansed on its passage through the layers of virgin soil, or washed down from a melting snowbank or glacier perched on a mountain peak, can a person feel safe in drinking from a spring, pool, lake, or stream.

Water has certain characteristics and behavior patterns that distinguish it from other natural products. By far the greatest source of water is the moisture-bearing winds. Unlike trees or grass, whose production can be diminished, maintained, or even increased by man's intervention, no way has been found to increase the amount or change the character of the precipitation that falls upon the earth, except experimentally and on a very much restricted scale. (Climatological investigations are raising a suspicion, however, that the exposure of large areas of forests and grassland by clearing for agriculture, towns, airports, and highways may be partly responsible for the less favorable rainfall conditions of recent times.)

Like soil, water can be productive and support healthful and prosperous communities provided its energy or flow is properly controlled and wisely used. But also, like soil, the flow and behavior of water when uncontrolled or unwisely used, may cause great distress, and impair or destroy, sometimes rapidly, the livelihood of communities. In fact, our treatment of the soil and its plant cover determines to a great extent whether water becomes friend or foe.

Because water is such an intimate part of our daily lives, most of us give little thought to it. Even in the drier Western states, people have been concerned almost entirely with water rights and water developments and have ignored the distant forest and range uplands whose condition determines the amount and quality of usable water that will become available to them. Few city folk have any idea of—and much less care about—the source of their water supply. Only when a crisis looms, as during extended drought or disastrous flood, does the stunned or annoyed populace become conscious of its water problems. And then it usually prefers to apply only temporary or stopgap measures aimed

Hoover Dam, downstream face. The usefulness of the 115-mile-long reservoir above it is steadily being diminished by silt washing down from the eroding Colorado River watershed. (Bureau of Reclamation photo.)

Columbia River flood scene, June 1948. Floods are perhaps the most aggravating type of water problem that has plagued the United States in recent decades. (U. S. Forest Service photo.)

at eradicating the immediate, superficial causes of distress.

The United States has, in fact, never faced up squarely to its water problems. Our policies, whether activated by Federal, state, or municipal agencies, have been to a large extent a potpourri of mutually conflicting measures, one community often striving to obtain benefits that prove harmful to others or even to an entire region.

Our water problems, like the land problems with which they are intimately related, are the product of civilized man's constant efforts to adapt his physical environment to his economic and social needs. In other words, they are the end result of man's lack of foresight, combined with greed and indifference to the welfare of his fellows. They are also, to a large extent, the result of ignorance of the laws of nature, as well as a refusal to adjust human institutions to conform more closely to these laws.

TOO MUCH OR TOO LITTLE WATER

Floods are perhaps the most aggravating type of water problem that has plagued the United States in recent decades. Soil-depleting agricultural practices, the burning and misuse of forest and grazing lands, and the general laxity in preventing erosion have contributed to devastating inundations and heavy debris and sediment movements over large parts of the country. A key factor has been the settlement and overdevelopment of vulnerable bottom lands, especially in the densely populated Eastern and Far Western states. The flood plains and channels of the Ohio, Mississippi, Allegheny, Connecticut, Delaware, Potomac, lower Missouri, Susquehanna, Willamette, Columbia, Sacramento, and other rivers, as well as their tributaries, have been increasingly encroached upon by real-estate developments, manufacturing, industrial and business establishments, and intricate transportation and communication networks. At the same time, the uplands have been increasingly abused by improper clearing, plowing, grazing, or lumbering, causing rain and melting snow to cascade into and overload the stream channels and tear up their once stable banks and bottoms. Over large parts of the United States nature's balance has been upset all the way from hill and mountaintop to valley floor, and despite large-scale costly measures the toll of flood and sediment damage has mounted steadily, and in recent years has averaged some $300,000,000 annually in property, business, and crop losses alone.

The flood problem is intimately related to the

growing inadequacy of many streams for navigation. Increasingly dams must be built on our major river systems to help smooth out the erratic seasonal flows; unstable river bottoms must be dredged to prevent channels from filling up and forming sand or gravel bars; and riprap and other devices must be constructed to protect riverbanks against erosion.

Nearly all these, however costly and imposing, are by themselves usually stopgap measures. In a great many localities, dams and other channel restrictions intensify rather than solve streamflow and sediment problems, as for example, along the Rio Grande following the construction of the massive Elephant Butte Dam in New Mexico, along the Colorado River after the building of gigantic Hoover Dam, and within the Grand Coulee Reservoir on the Columbia River. In fact, no permanent correctives for unstable channels or floods can be developed until the watersheds of our more troublesome river systems are dealt with as integral units. Local water problems are usually part of a vast pattern, and we cannot successfully cope with any facet—floods, sedimentation, water shortages, etc.—unless the basic evil—abuse of watershed lands and their waterways—is corrected.

Just as some parts of the United States are suffering from a surplus of water, others are struggling with perennial shortages. Scarcity of drinkable or otherwise usable water is fast becoming the limiting factor in the expansion of agriculture and industry and the growth of many communities in every part of the nation.

Only fifty years ago in the humid sections of the United States the natural flow of almost any large stream could be tapped to provide, with little treatment, ample quantities of "sweet" water, even in dry seasons. Today such favorable conditions are practically nonexistent. Underground water levels have receded in many developed or settled sections, forcing us to sink wells to great depths, with often less water for our pains than in the days of shallow wells. The deep wells, moreover, require expensive pumping equipment, which entails heavy operating costs for fuel oil, coal, or electric energy. And the water obtained in many instances

The life of an increasing number of water-supply reservoirs—on which farms, cities, and towns depend—is threatened by excessive siltation. (U. S. Soil Conservation Service photo.)

is of unsuitable chemical quality if "pure," or it contains salts or pollution that have seeped in.

There are few communities of any size that can depend any longer upon local, natural stream flow for their normal water supply. Municipalities and towns must constantly be on the alert for new sources and must incur larger outlays for storage reservoirs, aqueducts, and treating plants. Such problems are being faced, sometimes acutely, by Los Angeles, Baltimore, Tucson, Chicago, Louisville, New York, San Francisco, San Diego, Santa Barbara, Philadelphia, and scores of other municipalities. And many a city or town that only a few years ago had adequate reservoir capacity now finds that its resources have been depleted by siltation from eroding watersheds or by insufficient inflow during the dry season from disturbed mountain streams. Thus it is conservatively estimated that because of siltation 21 percent of the nation's 2,700 water-supply reservoirs have a useful life of under fifty years. In the Southeastern states, where the rate of siltation is high, 33 percent of the reservoirs face complete loss of usefulness within a half century.

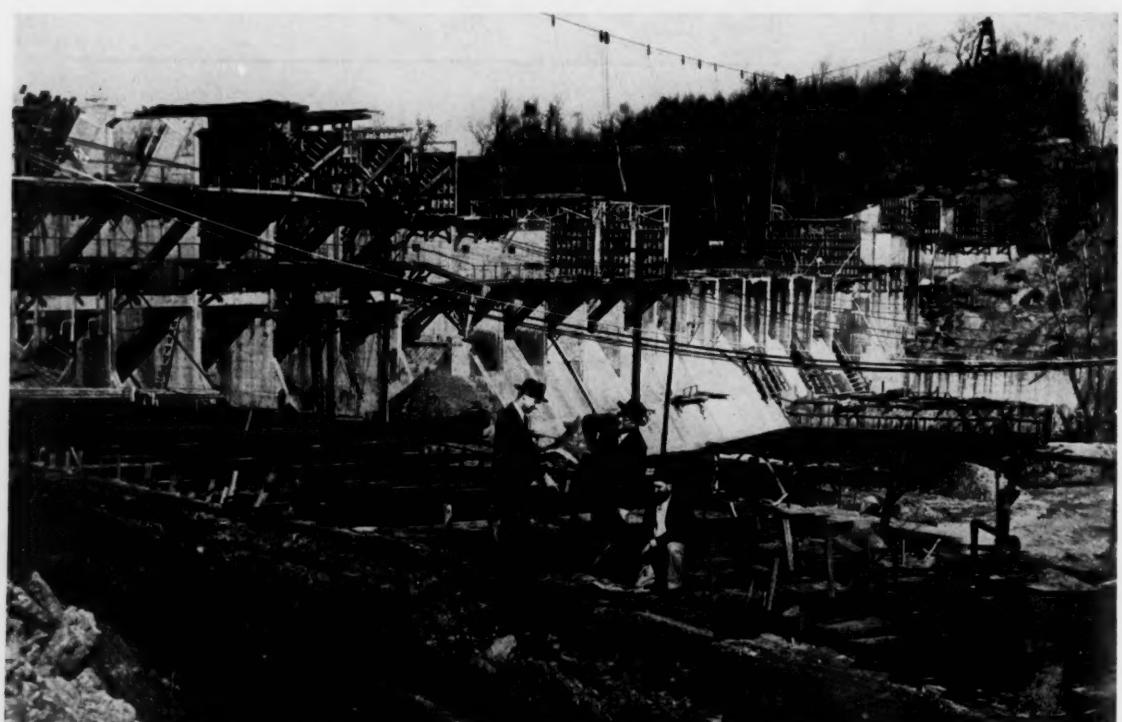
As a matter of fact, the nation's preventable water problems generally are destined to increase, perhaps in geometric ratio, if remedies are not applied on a unified watershed basis from the uppermost headwaters to the mouths of the major rivers. So far not a single watershed of any size in the United States has been so treated.

DEMAND FOR WATER OUTRUNNING SUPPLY

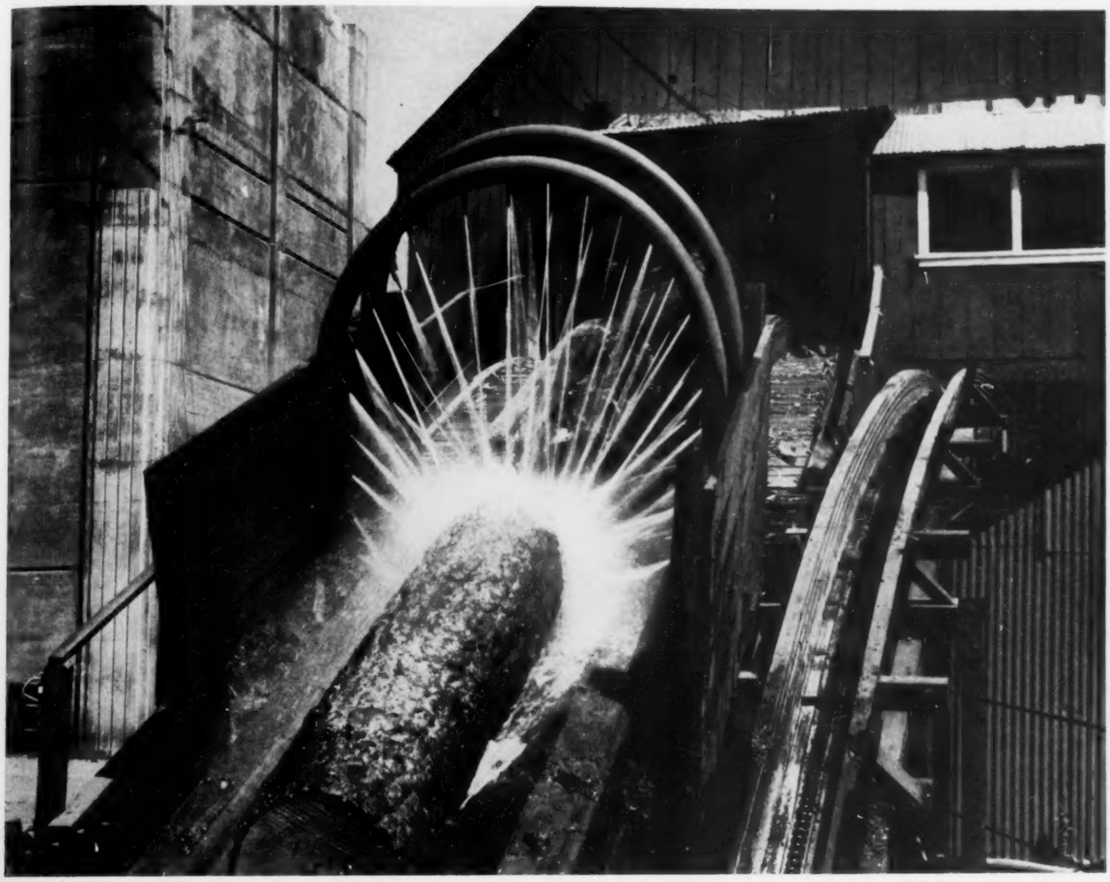
Although both the quantity and quality of our water supplies are declining, demand is steadily rising in response to population growth, industrial progress, expansion of agriculture, and technological improvements. Such developments as air conditioning and the spread of rural electrification create greatly augmented needs for water. Likewise, as the irrigated acreage increases and farming becomes more intensive and diversified, much more water is required. Also, rising health and living standards inevitably mean higher per capita water consumption.

A hint of the phenomenal growth in the demand for municipal water may be obtained by the increase in water consumption in a large city like Chicago. In 1880 the rising municipality of Chicago, with a population of 500,000, consumed around 140 gallons per person per day, or some 26 billion gallons per year. By 1940 the 3,400,000 inhabitants of Chicago were using about 290 gallons per person per day, or some 360 billion gallons every year.

The rapid growth of cities has indeed put a



Brighton Dam, Washington Suburban Sanitary Commission. Water for drinking and other domestic purposes is obtainable only at considerable outlay of labor and materials. (U. S. Soil Conservation Service photo.)



Water enters intimately into all industrial processes in a great variety of ways. The growing shortages of water threaten the existence of many industries. (U. S. Forest Service photo.)

tremendous strain on our water supplies, but expansion of irrigation agriculture has added perhaps even greater burdens. Public and private irrigation projects now under way or projected call for the development of nearly all the water supplies still "unused" west of the Mississippi River. The area in irrigated crops has grown from less than 8 million acres in 1900 to nearly 20 million acres in 1940 and 22.5 million acres in 1948; under current authorized plans, more than 7 million acres of new land in the 17 Western states will be supplied with water by the Bureau of Reclamation, and supplemental water will be brought to more than 3.5 million acres now inadequately supplied.

The expansion of irrigation agriculture creates a large *indirect* as well as direct drain on our limited water supplies. New rural and urban communities spring up in the reclaimed areas; and business, transportation, and industrial enterprises arise to process and ship the agricultural produce and to provide farm supplies, equipment, and community services. All these require water for their very existence.

As a result of rapidly increasing demands, competition for "water holes" is rife in some parts of the United States. Industrial or municipal demands are infringing upon the needs of agriculture in Utah, Arizona, California, and even Virginia. Some states have taken up arms against other states to protect their water rights—and cities are locked in combat with other cities. Communities in southern California and central Arizona are disputing bitterly for rights to Colorado River water despite a compact, reached after much bickering, allocating the supplies impounded by Hoover Dam and its 115-mile reservoir, Lake Mead. Los Angeles is barring the towns on its periphery from tapping its water mains, so short is the supply for the mushrooming city. Demand for water here has already reached a point which engineers before World War II did not expect before the year 2000. Municipalities like San Diego, with war-swollen populations, find that demand is constantly outrunning resources, and the search for clean, usable water becomes ever more intense. Philadelphia's avowed intention to

Industrial and silt pollution on far too many Eastern and Western streams is destroying fisheries and otherwise rendering the water unfit for use downstream. (U. S. Forest Service photo.)

increase its draft on the Delaware River is causing alarm in New York as well as in New Jersey cities. New York City's expressed desire to tap the Connecticut River is arousing the ire of Boston, and its claim to rights on the Delaware watershed has already resulted in litigation with the city of Philadelphia.

The expansion of our economy will inevitably intensify the quest for dependable water supplies. For example, a major battle has developed in the far-flung Missouri River Basin between economic interests of the lower valley, who demand adequate stream flow for navigation, and those on the upper reaches of the river and its tributaries, who seek water for irrigating new farm land. The flow of the vast Colorado River is being diverted in several directions—to the Missouri River in the east, and to the farms and cities of the Intermountain Basin, Imperial Valley, and southern California on the west, thus vitally affecting the often conflicting economic interests of Utah, Colorado, Arizona, California, and even northern Mexico.

In some parts of the United States there are already insufficient water supplies to support the present population, industries, and irrigation agriculture. For example, the building of the huge Geneva steel plant at Provo, Utah, provided a shot in the arm for the economy of the region, but it also created a tremendous water problem. The Weber River, whose waters are diverted to this area, cannot indefinitely supply the towns, the steel mill, and the farm lands dependent upon it. Likewise, the Los Angeles area will soon have to decide between airplane factories and orange groves, since the available sources of water cannot indefinitely supply both at reasonable cost.

Other Western regions are in a similar plight. In the San Joaquin Basin of California the usable water supply fails to meet the needs of the 2 million valuable acres in irrigated farms which produce, among other crops, luscious fruits for the discerning palates of Eastern as well as Western people. In years of abnormally low rainfall, low waters in the streams bring crop failures or force farmers to leave the land fallow. The rivers cannot be navigated, and saline waters from San Francisco

Bay seep into the wells of farms on the coastal plain. During such periods ground waters are overdrawn and water tables are lowered, causing pumping costs to zoom.

In the Salt River area in Arizona, water users are threatened with the loss of the vast storage capacity of Roosevelt Reservoir, on which one tenth of the state of Arizona, especially the highly productive Phoenix region, is dependent. Here sediment is washing down from the overgrazed, highly erosive granitic soils of the mountains and foothills into the channels and reservoirs.

The Gila River watershed in Arizona and New Mexico presents another challenge to man's ingenuity in restoring our water resource. In this region of 17,000 square miles, floods have become more frequent and destructive in recent years, ground waters are ebbing materially, and the large San Carlos irrigation reservoir is silting up. Large portions of the watershed have been stripped of their grass and brush cover, with the result that flash floods are more common in winter and heavy movement of debris in summer.

In these areas, as elsewhere, uncontrolled or uncoordinated use of the land and its water resources by diverse interests cannot continue without harming the entire economy. Even in the Columbia River watershed, despite vast sources of relatively untapped water supplies, conflicts have already developed between flood-control and irrigation interests on the one hand, and commercial fishermen on the other. Also, California is now considering the possibility of tapping the distant Columbia's flow to augment the supply from the Sacramento River. Industrial and silt pollution, however, on

Snow survey investigations, Crater Lake, Oregon. Too few people give any thought to the source of their water supply in distant forest uplands. (U. S. Soil Conservation Service photo.)

tributaries of the Columbia River, as on the Willamette, for example, is rendering the water unfit for use further downstream without costly treatment. On the uplands, unsatisfactory agricultural practices, overgrazing, the effects of past fires, and accelerated logging and road building are resulting in high rates of snow melt, surface runoff, and sedimentation and debris movement, as was so forcibly demonstrated by the flood of May–June 1948.

In short, so long as present land-use trends continue, the nation's hill and mountain areas face a steady decline in their ability to furnish well-regulated supplies of usable water to farms, cities, and towns dependent upon them, or to restrain runoff that annually floods tilled and populated bottom lands, with catastrophic results.

A FOREST REAPPRAISAL

LYLE F. WATTS

Mr. Watts (M.F., 1928, Iowa State) is Chief of the United States Forest Service. He has been with the Forest Service since 1913 except for a year as professor of forestry at Utah State College.

SEVENTY-SIX years ago the American Association for the Advancement of Science, at its meeting in Portland, Maine, took note of the alarming rate of exploitation and depletion of the forest resources of the United States. It appointed a committee to memorialize Congress and the state legislatures, urging action to stop the wanton destruction of American forests and to make provision for the proper use and perpetuation of the forest wealth remaining.

Three years later, in 1876, Congress gave a modest bit of recognition to this request. To a section of the appropriation bill granting funds for the distribution of seed, Congress added a provision that

> . . . two thousand dollars of the above amount shall be expended by the Commissioner of Agriculture as compensation to some man of proved attainments, who is particularly well acquainted with methods of statistical inquiry, and who has evinced an intimate acquaintance with questions relating to the national wants in regard to timber, to prosecute investigations and inquiries, with the view of ascertaining the annual amount of consumption, importation, and exportation of timber and other forest products, the probable supply for future wants, the means best adapted to their preservation and renewal, the influence of forests upon climate, and the measures that have been successfully applied in foreign countries, or that may be deemed applicable in this country, for the preservation and restoration or planting of forests; and to report upon the same to the Commissioner of Agriculture to be by him in a separate report transmitted to Congress.

That was a large order, even for a man of "proved attainments." The man selected for this comprehensive assignment was Dr. Franklin B. Hough, who had headed the AAAS committee that prepared the memorial to Congress. Dr. Hough set diligently to work. He traveled over the country visiting lumber districts, tanneries, and tree plantations. He interviewed governors and other officials of many of the states, and circularized land offices and manufacturers using wood. A year after he took office he was ready with his report. In spite of its 650 pages, Congress ordered a printing of 25,000 copies.

His appointment being continued, Dr. Hough completed a second report in 1878, and a third, after a trip to Europe, in 1882. His three reports were monumental pieces of work. They gained wide attention both in this country and abroad; they were awarded a diploma of honor at an international geographical congress in Vienna. They brought together more information on American forest resources than had ever before been assembled. They helped greatly to stimulate the development of a forest conservation movement in the United States. From the small acorn planted by the AAAS, an oak began to grow.

It was of course impossible for any one man to obtain complete and accurate information on the extent and condition of forests and forest lands covering nearly half a continent. We still lack complete, detailed, up-to-the minute information even today. Since Dr. Hough's reports, a number of other reports have been compiled, each adding to our understanding of the forest situation in America. In 1909, the Bureau of Corporations made a study of lumber supplies and uses. The "Capper Report" of 1920 was prepared by the U. S. Forest Service in response to a resolution introduced by Senator Capper of Kansas calling for a report on timber depletion, lumber prices, lumber exports, and timber ownership in the United States. In 1933, the Forest Service prepared and sent to the Senate *A National Plan for American Forestry.* Popularly called the "Copeland Report," since it was published under authorization of a Senate resolution introduced by Senator Copeland of New York, this 1,677-page report brought together the largest amount of information on the forests of the United States that had been assembled up to that time. It also presented recommendations for nation-wide action to insure the economic and social benefits that could and should be derived from well-managed forest lands. In 1936, the Forest Service issued a report on the Western Range. Another comprehensive statement was prepared by the Forest Service in 1940 for a Joint Congressional Committee on Forestry that had

been established in response to a special message to Congress from the President. Following a three-year study, the Joint Committee issued a summary report on *Forest Lands in the United States.*

The McSweeney-McNary Act of 1928—the act which provided a legislative charter for a broad national program of forest research—among other things authorized a

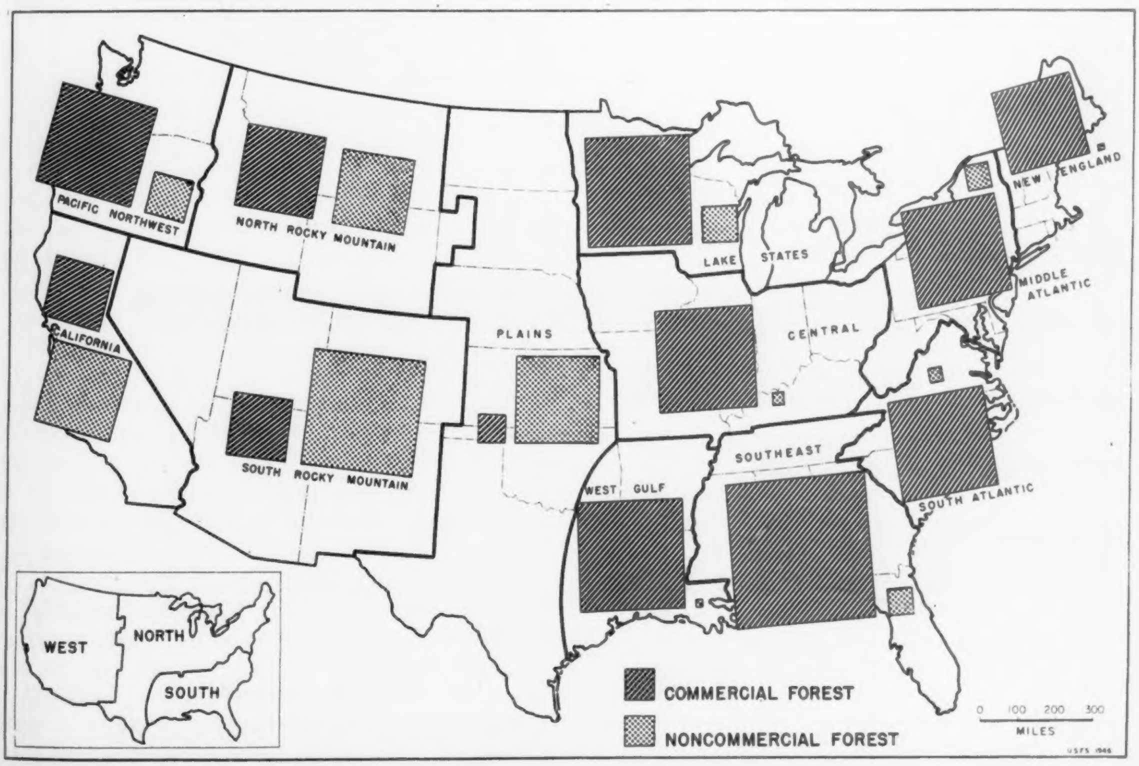
comprehensive survey of the present and prospective requirements for timber and other forest products in the United States, and of timber supplies, including a determination of the present and potential productivity of forest land therein, and of such other facts as may be necessary in the determination of ways and means to balance the timber budget of the United States.

Under this authorization, the Forest Service began in 1930 the first complete survey ever undertaken of forest resources and conditions on the nation's 631,000,000 acres of forest land. To date, initial field inventory work has been completed on nearly three fifths of the country's forest area. Since the war, resurveys have been made in several states where the initial surveys were made a decade or more ago or where there have been marked changes due to growth and cutting. Other phases of the Forest Survey have produced reports on present and prospective requirements for lumber for urban and rural housing, for shipping containers, and for several other classes of forest products. Completed and kept current, the nation-wide Forest Survey eventually will provide comprehensive and reliable data on forest resources in every state and region. Such authentic information is essential both for the formulation of sound public forestry policies and for business decisions of forest industries and landowners.

Meanwhile, another big step toward providing what Congress called for back in 1876 has been made in a postwar reappraisal of the forest situation in the United States, recently completed by the Forest Service. With the close of World War II, the Forest Service undertook to check up on current trends, evaluate progress in forestry, and provide an up-to-date factual basis for national conservation objectives and policies.

I

The forest reappraisal was a huge fact-finding job. It involved checking and supplementing large amounts of information available from the Forest Survey and from other sources. Much new resource information also was obtained to assure an adequate summary of the quantity, quality, distribution, growth, and drain of the timber re-



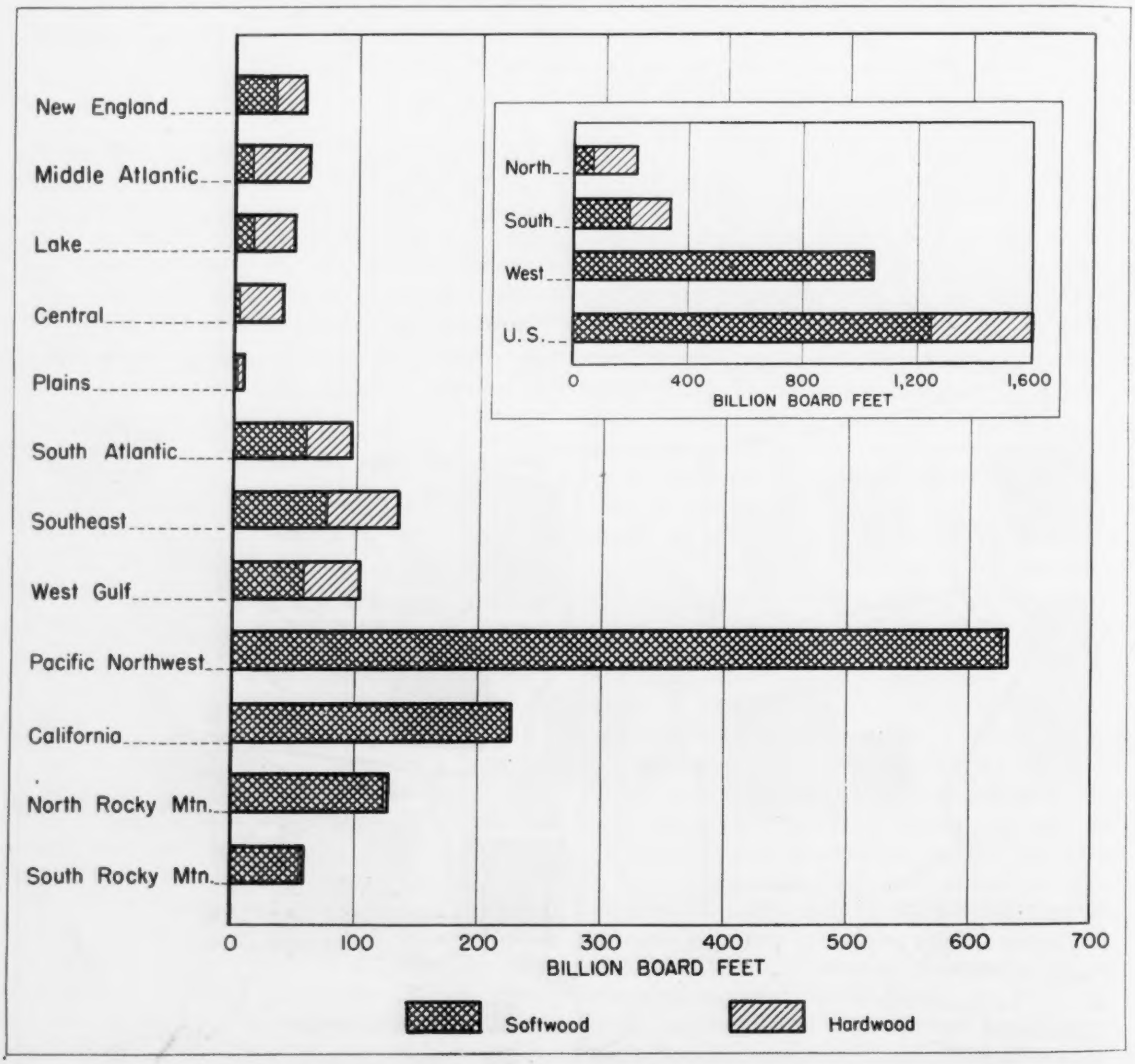
Distribution of the forest lands of the United States by regions.

sources. Estimates were made of potential requirements for forest products. Especially important new information on the character of forest practices and the intensity of forest management was obtained by a field survey. The volume and character of wood waste and the possibilities of using more of it were explored. Problems of the timber industries in relation to raw-material supplies were reviewed. The status and needs of forest protection were re-examined.

The reappraisal was conducted under the general direction of Raymond E. Marsh, Assistant Chief of the Forest Service. It was a Service-wide undertaking, with a large number of administrative and research personnel in the several regions and in Washington participating in one phase or another. The Division of Forest Economics and the regional forest and range experiment stations carried the main load. The Forest Service had the cooperation of other Federal agencies, state foresters and other state officials, and the American Forestry Association. Many private organizations and individuals contributed to the project.

An over-all report analyzing and interpreting the reappraisal findings has just been published under the title *Forests and National Prosperity*. Earlier, six reappraisal reports were issued, on the following subjects:

1. Gaging the Timber Resource of the United States.
2. Potential Requirements for Timber Products.



Saw-timber stand in the United States, by region, 1945.

3. The Management Status of Forest Lands.
4. Wood Waste.
5. Protection Against Forest Insects and Diseases.
6. Forest Cooperatives.

The forests are invaluable in the protection of watersheds. They have important values for recreation, for wildlife habitat, and for production of livestock. The reappraisal, however, dealt mainly with the timber resource.

The reappraisal findings give us the best picture we have yet had of the timber situation in the United States. The situation is such as to give cause for concern. The nation's saw-timber supply is declining, and its quality is deteriorating. Yet indications are that our annual timber growth is below what we really need now and is far short of what should be available in the future for a strong, expanding economy.

There is enough forest land in the United States, if well managed, *ultimately* to grow all the timber products we are likely to need, plus a margin for unavoidable losses, new uses, export, and national security. But our forests are not now in condition to do this.

Of the 624 million acres of forest land, about 460 million are classed as "commercial" because they are capable now or prospectively of producing merchantable timber and are available for that use. But about 16 percent (75 million acres) of this commercial forest land is now so denuded or so poorly stocked that it must be classed as idle land. Another 180 million acres supports only pole timber or seedlings and saplings, fairly well stocked. Saw-timber stands cover 205 million acres, of which 160 million are second growth of varying quality, and 45 million are virgin stands. Of this remnant of virgin timber, only one fourth is of high quality; more than a third is of doubtful commercial value.

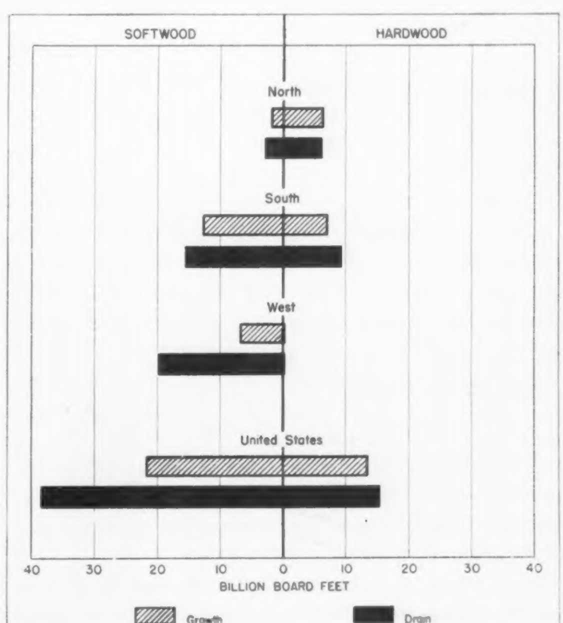
We now have less than 1,600 billion board feet of saw timber, and this is not well distributed. About one third is concentrated on the 6 percent of the commercial forest lands in western Washington and Oregon. The East, with three fourths of the commercial forest land area, does not have enough growing stock to sustain for long its present output.

The volume of standing saw timber in 1945 was 43 percent less than that estimated in 1909, and 9 percent less than in 1938. Just as important, the quality and size of the timber are deteriorating.

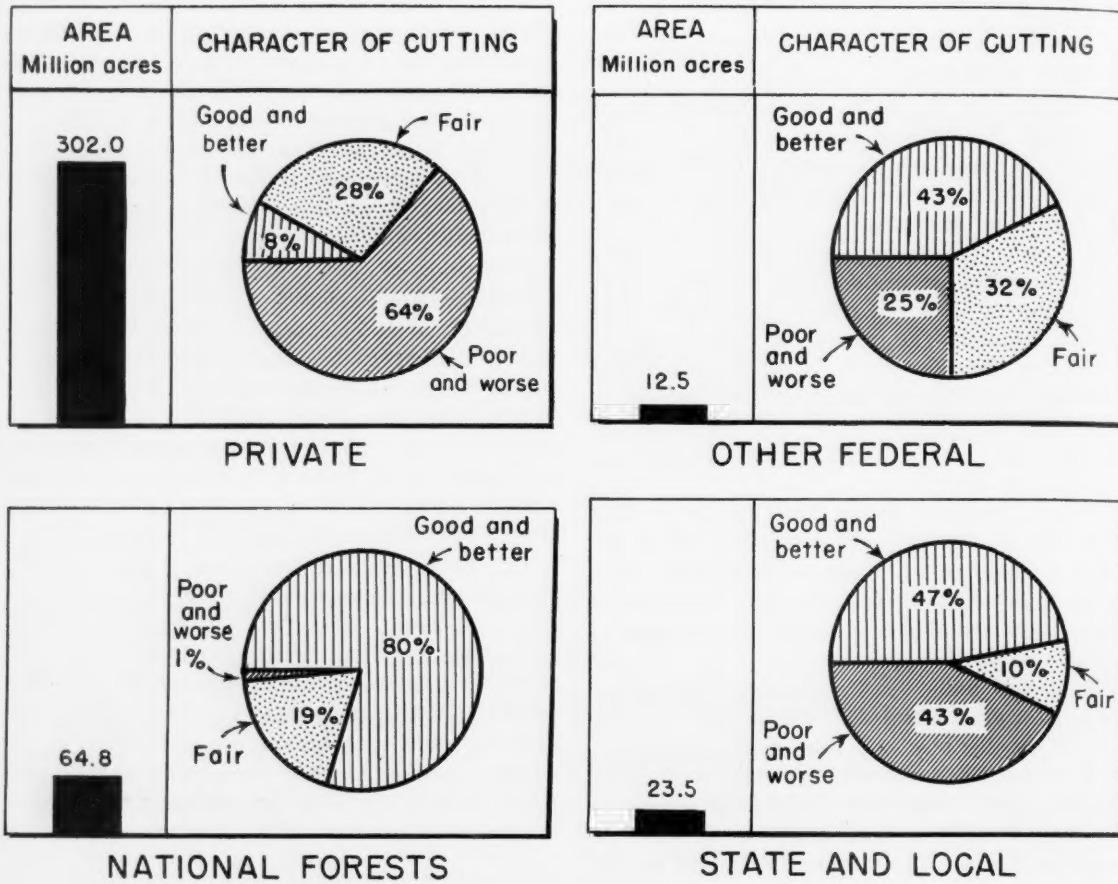
Saw-timber drain in 1944 was 53.9 billion board feet. It exceeded the estimated annual growth of 35.3 billion board feet by 50 percent. (Drain includes timber cut, plus natural losses; about 90 percent of the total drain is from cutting.) For all timber, including trees below saw-timber size and quality, the total drain was 13.7 billion cubic feet, compared with an annual growth of 13.4 billion cubic feet. But 80 percent of this drain is in saw timber, particularly softwoods, whereas much of the growth is in small low-grade trees and inferior hardwood.

Production of quality lumber and other quality saw-timber products has been affected by a growing scarcity of suitable, accessible timber. Shortage of high-quality timber has contributed to high lumber prices, which rose much faster than those of other building materials, and which undoubtedly are a deterrent to our using as much lumber as we should like to. There is much evidence that the intrinsic needs of the country for timber products are considerably greater than the present cut.

The forest situation thus poses a dilemma. To increase current output means an acceleration of timber depletion, especially in the East, that would hasten the day when drastic reduction in the use of timber products would be inescapable. To curtail Eastern output so as to facilitate building up growing stock and annual growth would leave urgent needs, such as that for housing, unfilled and would restrict the base for maintaining a high-level national economy. This country cannot rely



Growth and drain of saw timber, United States, 1944.



Operating area and character of cutting by ownership class, 1945.

to any great extent on imports from other countries because there is a world shortage of timber, especially of softwoods for construction. There does not seem to be any wholly satisfactory solution.

To help maintain national output, the cut of virgin timber in the West can be increased for a number of years. This will require rapid construction of access roads into undeveloped country, particularly in the national forests. Any increase in cut in the Western states should not be at the expense of good forestry practice, if future growth is to be maintained.

From the national forests of Alaska, whose resources are as yet largely untapped, we can eventually get about 7 percent of the nation's potential pulp and paper requirements.

Utilization of some of the wood now wasted in harvesting and processing can help bridge the gap, though it cannot decisively relieve the pressure on our growing stock. At present less than half the weight of the wood we cut or destroy in logging shows up in finished products.

About one third of the nation's total volume of standing saw timber is now in the national forests, although the national forests comprise only 16 percent of the country's commercial forest area. Thus these public forests assume large importance in cushioning the decline in output of forest products which we face in the years before the needed productivity of the forests as a whole can be built up. Output of the national forests should be developed as rapidly as possible, but it becomes doubly important to safeguard their future productivity by keeping the cut within their sustained-yield capacity.

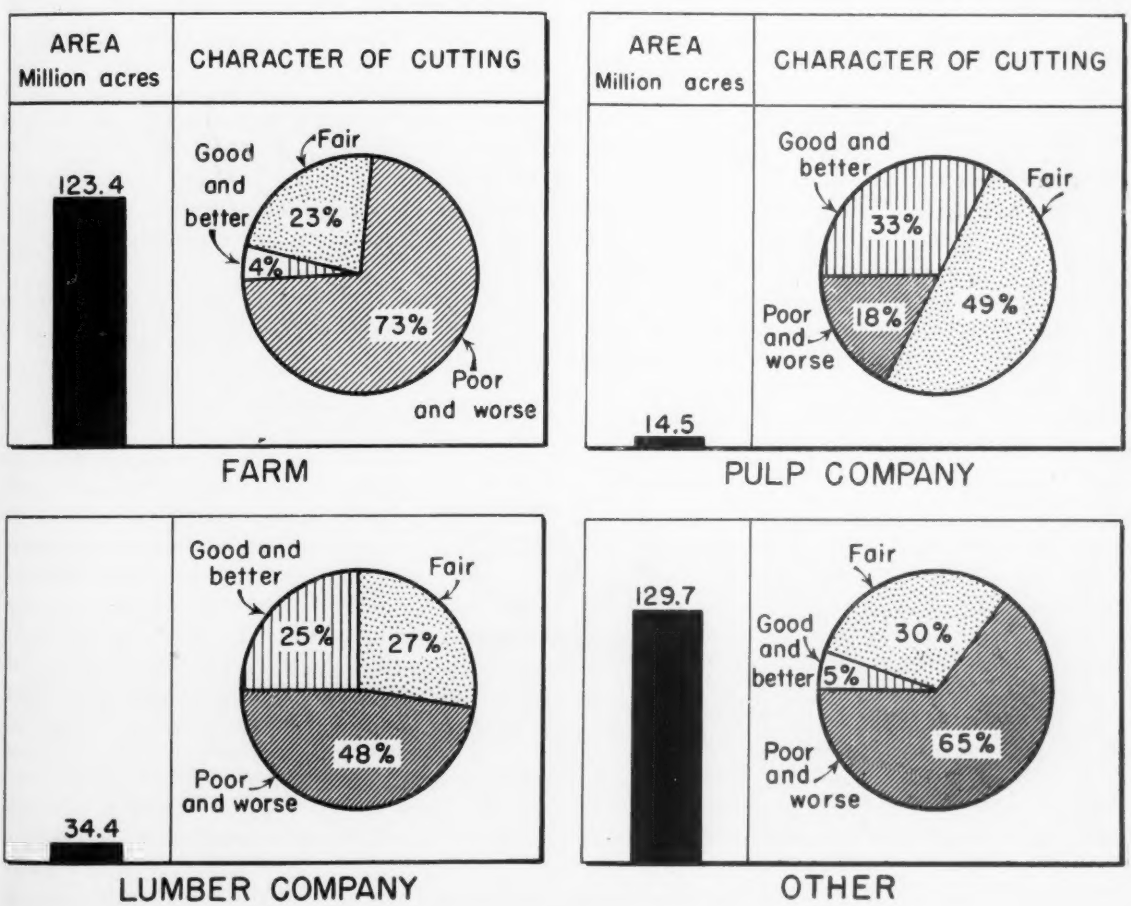
With only 16 percent of the country's commercial timberland, however, the national forests certainly cannot supply all our requirements for wood. We must depend mainly on the private land.

Timber-cutting practices on these lands, with some notable exceptions, are far from satisfactory

Encouraging improvement has been made in recent years, especially by some of the larger owners, but about two thirds of the cutting is still poor or destructive; only 8 percent is up to really good forestry standards. The larger properties, chiefly lumber- and pulp-company holdings, receive the best treatment. But these comprise only 15 percent of the private commercial forest land. Three fourths of it, about 261 million acres, is in more than 4 million small properties averaging 62 acres each. About half of these small forest properties are farm woodlands. On the small forest holdings, farm and nonfarm, about 71 percent of the cutting is poor or destructive. Improvement of forest practice on these millions of small private holdings is an especially difficult problem.

Careful study—looking beyond current limitations to long-range needs—indicates that 65–72 billion board feet would be a reasonable goal of annual saw-timber growth to meet prospective future requirements. That will mean doubling the current rate of saw-timber growth, which is a big order. Even if there could be prompt and widespread adoption of good cutting practices and other forestry measures—which only the most optimistic among us would expect—it would probably take at least until the end of this century to build up the needed growing stock. But to aim for less would not be sound public policy nor consistent with the responsibilities and needs of the nation.

In presenting the findings of the Forest Reappraisal, the Forest Service recommended certain measures that it believes essential to achievement of national timber growth goals. Although those measures are concerned primarily with timber production, they will go far toward meeting related needs—toward safeguarding watershed, range, scenic, recreation, and other values that, in some regions, transcend that of timber supply. Some of the measures call for new authority; others simply



Private operating area and character of cutting by type of owner, 1945.

require more adequate implementation of current activities. In brief, they are:

First, an extension of public aids to private forest landowners, including:

a) Technical advice and assistance to private owners in establishing and tending forests and in harvesting and marketing forest products, and corresponding advice and assistance to operators of small wood-processing plants.

b) A federally sponsored forest credit system to make long-term loans on terms and conditions suitable for forestry purposes.

c) Encouragement of forest cooperative associations as a means of achieving good forest management, particularly on small holdings.

d) Acceleration of forest planting on private land.

e) Extension and intensification of cooperative fire protection.

f) More prompt and adequate detection and suppression of incipient epidemics of forest insects and diseases.

g) Advisory service to aid states in the improvement of forest tax laws and their administration.

Second, public control of cutting and other forest practices on private lands sufficient to stop destructive practices and keep these lands reasonably productive. It is just as important to protect the forests against destructive cutting as it is against fire. The Forest Service believes that adequate regulation could be accomplished by a Federal-state plan that would assure nation-wide application of the same standards but would give opportunity for state action and for Federal financial assistance.

Third, intensified development and management of the national forests. This includes among other measures, forest planting, timber-stand improvement, and access roads to open up hitherto inaccessible working circles and expand the contribution of national-forest timber to the nation's needs. Beyond this, substantial areas partly within existing national-forest boundaries either should be placed under public ownership and administration because they are submarginal for private ownership, or they should be acquired for other reasons of public interest, such as watershed protection. State and community forests also have an important part to play in public ownership and management.

An important corollary is that, commensurate with this country's growing responsibility in world affairs, we should work actively to encourage international cooperation in forestry. And to back up the needed program of action, we must of course have continued and intensified research in forestry. More research will be needed to make sure we have the best answers to the many problems of forest management, protection, and utilization, and to find the answers to problems yet unsolved. Through research we can find even better and faster ways of growing timber. We can find ways to reduce waste and make the timber cut go farther.

Our forest land area is capable of producing timber in sustained abundance. The challenge is to make it produce.

SCIENCE ON THE MARCH

FRACTURE OF LIQUIDS: NUCLEATION THEORY APPLIED TO BUBBLE FORMATION

CALIFORNIA redwood and sequoia trees (Fig. 1) grow to a height of 300 feet, and the water that evaporates daily from their leaves must be raised this great distance from the soil. Water-carrying passages in the trunks of the big trees are relatively large; the capillary rise in them is a matter of inches. How, then, is the water lifted a hundred yards into the air?

One might suppose that water is forced to the tops of trees by pressure at the roots, as sap is forced from holes bored in the trunks of sugar-maple trees early in the spring. If root pressure were responsible, a pressure of about 10 atmospheres would have to be present in the sap at the base of a sequoia tree in order to support and lift a column of fluid 300 feet high. Were a hole bored in the tree, a strong flow of sap would be expected. Yet when such a hole actually is made at the base of a sequoia tree (or of any other tree in full leaf), nothing comes out of the opening. On the contrary, water poured into the hole moves into the tree and later appears in the leaves. The sap pressure at the base of the trunk is therefore *less than atmospheric,* and a mechanism other than root pressure must operate to supply the leaves with moisture.

Water that arrives at the tops of trees is in reality pulled up by the leaves. As moisture evaporates from leaf cell surfaces, cell walls resist the inward pull of their shrinking contents; the pressure of the water in the leaves drops. It becomes less than atmospheric, then less than zero, and finally reaches a large negative pressure. When the negative pressure in the leaves of a 300-foot tree exceeds 10 atmospheres, the pressure in the roots becomes negative also, and water from the soil diffuses into the roots. The upward motion of water in a tree therefore depends upon the presence of a sufficiently large negative pressure at the treetop to maintain a lower diffusion pressure of water in the roots than in the surrounding soil.

Only in early spring, when the soil is saturated and the diffusion pressure of soil water is high, and when—more important—there are no leaves from which evaporation can occur, does the sap pressure become positive. The sap flows from sugar-maple trees only after the spring thaw and before the first leaves appear.

Negative pressures are present in most plants that grow in air. For example, 10 atmospheres negative pressure in corn plants is frequently attained on dry sunny days. A hundred atmospheres of negative pressure have been measured in seeds. Were it not for the fact that water can withstand a large hydrostatic tensile force in the absence of free surfaces, most land plant life could not exist in its present form.

Liquids with free exposed surfaces are known to boil when their pressures are reduced below the vapor pressure, yet tree sap does not boil even at pressures far less than a vacuum. The difference lies in the difficulty of creating a bubble of vapor inside a liquid.

Figure 2 shows a small vapor bubble in a liquid under negative pressure. The liquid is contained in a rigid tank, and the negative pressure is maintained by a weight suspended from a frictionless piston. A definite potential energy change is associated with the formation of the bubble:

1. A surface is created between the bubble of vapor and the surrounding liquid. Work is required to form a surface against the force of surface tension which tends to shrink it out of existence, and the potential energy increases by the amount of this work.
2. The weight falls until the volume of the container behind the piston has increased by the volume of the bubble (less the usually negligible volume that the vapor in the bubble would occupy if condensed). The potential energy of the system decreases as the weight falls.

The total potential energy change associated with the formation of a bubble of radius r is plotted versus bubble radius in Figure 3. It can be seen that the formation of little bubbles having relatively large surfaces requires an increase in potential energy. On the other hand, a potential energy decrease accompanies the formation of larger bubbles which have relatively small surfaces. Bubbles of critical radius r^* require the greatest potential energy increase.

Does a bubble tend to grow or to shrink? The answer depends upon its size. Bubbles with radii less than r^* require energy for further growth, whereas those with radii larger than r^* grow with deceasing energy. Since bubbles grow larger or smaller one atom at a time as the result of sta-

tistical thermal fluctuations, it is evident that small bubbles with radii less than r^* usually will disappear without reaching the critical size. Only rarely will a long chain of favorable energy fluctuations produce a bubble exceeding the critical size in a liquid initially free of bubbles. When this unusual event does happen, however, the supercritical bubble grows with increasing rapidity; the liquid boils.

A cup of liquid contains about a million million million million molecules, each of which is moving around so that it hits its neighbors about 10 million million times a second. Tiny bubbles appear unavoidably from time to time here and there in the liquid, as the random dance of molecules continues. In time, a supercritical bubble will appear spontaneously in a liquid subjected to any given negative pressure.

FIG. 1. Giant redwoods soar 300 or more feet in the air.

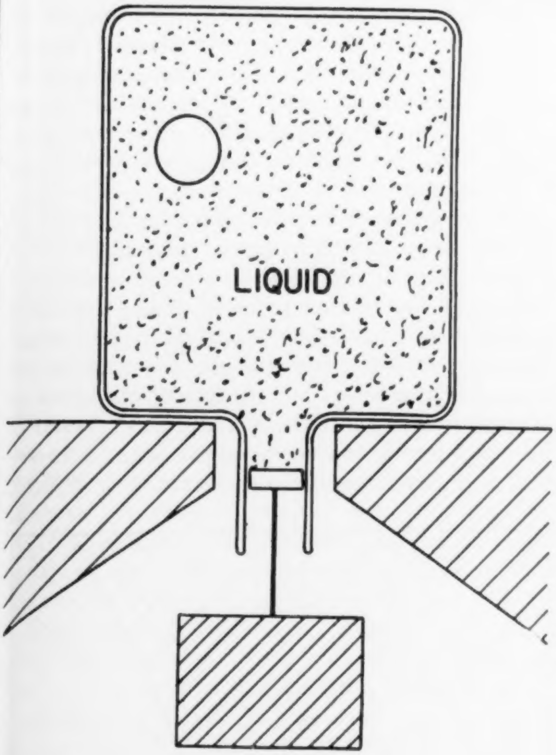


FIG. 2. Small vapor bubble in a liquid under negative pressure.

The theory of nucleation, which deals with spontaneous processes of this kind, gives an expression for the rate of formation of bubbles of vapor in a liquid subjected to negative pressure (assuming, of course, that somehow the pressure is maintained even after the first bubbles have begun to grow).

From this expression it is fairly easy to estimate the negative pressure at which a liquid will fracture (boil). Since the first bubble that forms fractures the liquid, the fracture pressure of the liquid will be that which gives one bubble in a reasonable time—say, one bubble a second. For water at ordinary temperatures the fracture pressure is 1,300 negative atmospheres. If the fracture pressure is computed for one bubble a year rather than one a second, the fracture pressure for water is reduced only to 1,180 negative atmospheres—still enough to lift water to the top of a tree 40,000 feet high.

Measured values of the fracture pressure of water range from the positive vapor pressure to a negative pressure of about 350 atmospheres. The highest experimental negative pressure that water has withstood is therefore only about 30 percent of the theoretical value. However, the theoretical fracture pressure was derived assuming that the vapor bubble responsible for fracture was formed in the interior of the liquid. The low experimental values suggest that the initial vapor bubble may form instead at the interface between the water and the container.

A vapor bubble at the interface between a liquid and a plane solid surface assumes a shape bounded by a plane and a portion of a spherical surface, as shown in Figure 4. In the figure, σ_{lv}, σ_{sv}, and σ_{sl} represent the liquid-vapor, solid-vapor, and solid-liquid surface tensions.

For certain values of the three surface tensions σ_{lv}, σ_{sv}, and σ_{sl}, it is much easier to nucleate vapor bubbles at a solid-liquid interface than in the body of the liquid. Although new liquid-vapor and new solid-vapor interfaces are created bounding the bubble, the solid-liquid interface is destroyed over that portion of the solid surface touched by the bubble. If the solid-liquid surface tension is high enough, sufficient energy is made available by destroying the solid-liquid interface to supply much of the energy needed for both the liquid-vapor and the solid-vapor interfaces of the bubble. Then little or no net energy is required for the formation of the bubble surfaces, and the liquid will fracture more easily. A tiny crack or other irregularity in the solid at the solid-liquid interface tends to re-

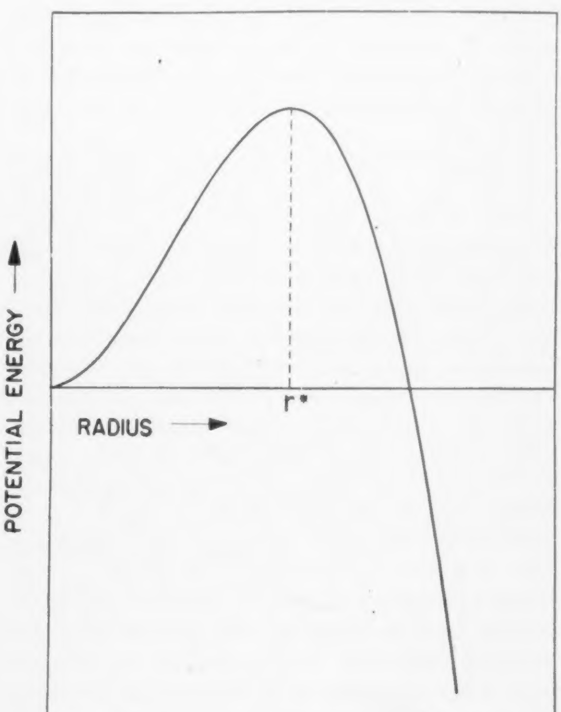


FIG. 3. Energy of forming a bubble of radius r versus bubble radius.

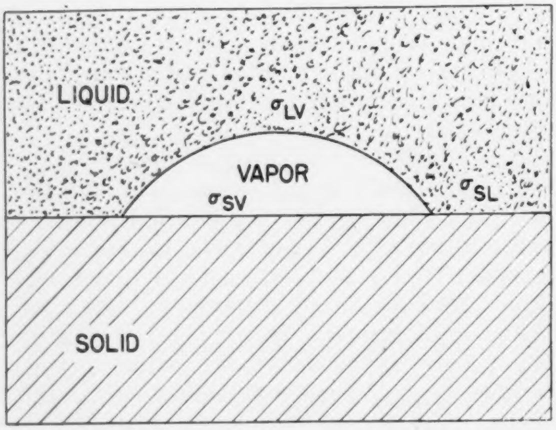


FIG. 4. Vapor bubble at interface between a liquid and a plane solid surface.

duce the magnitude of the fracture pressure still further.

Since the solid surfaces of the containers in which the fracture strength of water has been measured probably had widely differing values of σ_{sl} (a small spot of impurity may lead to a very large local value of σ_{sl}), one ought to find experimental fracture pressure values for water lying anywhere between the vapor pressure and the theoretical value for internal bubbles. The wide scatter of observed fracture pressures can be interpreted satisfactorily in this manner. The full 1,300 negative atmospheres can be realized only when the solid-liquid interfacial tension is sufficiently small.

Undercooled liquids (amorphous solids), such as glass, also will fracture when subjected to high negative pressures. Cracks appear in glass instead of bubbles as for liquids; otherwise the fracture mechanism is quite similar. The theoretical value of the fracture pressure for glass at room temperature is about 130,000 negative atmospheres, a value that checks quite well with the highest observed fracture strength of about 100,000 negative atmospheres for tensile tests of fine glass fibers. Most ordinary glass fails at very much smaller stresses by the growth of pre-existing surface cracks resulting from accidental damage.

When the fracture of liquids or amorphous solids is thought of as resulting from the nucleation and growth of a bubble or crack, a theoretical fracture strength value can be derived from the theory of nucleation. The theoretical strengths so found are, as they should be, larger than the highest measured values; yet they are not unreasonably larger. On the other hand, the theoretical strength calculated from the force necessary to separate planes of atoms in the absence of bubble or crack nucleation is an order of magnitude greater than that derived from nucleation theory; hence, there can be little doubt that such separation of planes of atoms is not the actual fracture mechanism for liquids or solids—the nucleation and growth of bubbles or cracks is a much more promising mechanism.

The fracture of metals and other crystalline solids is more difficult to analyze than the fracture of glass. Metals usually bend or yield before they break, and the regular patterns of the thousands of individual crystals or grains of which an average piece of metal is composed are distorted as deformation proceeds. Fracture usually originates as a crack in a region where a distorted crystal is subjected locally to a negative pressure far in excess of the average applied pressure. It is difficult

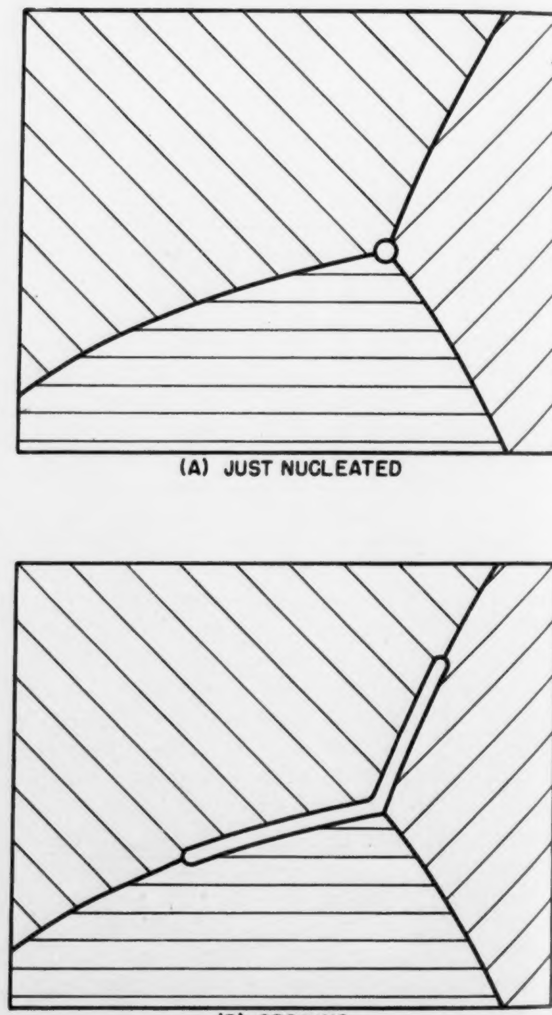


FIG. 5. Progress of a bubble along a grain boundary (schematic).

to estimate the magnitude of the stress concentrations that occur in severely distorted crystals, and the theory of fracture is incomplete for crystalline solids that deform before they break.

At very high temperatures, however, metals pull apart at the boundaries between grains. High temperatures give mobility approaching that of liquids to the atoms at grain boundaries, and a metal subjected to negative pressure fails by nucleation and growth of bubbles at grain boundaries, just as a liquid fails by nucleation and growth of bubbles at the container wall. The progress of a bubble along a grain boundary is illustrated schematically in Figure 5.

A marked decrease in the strength of metals is observed at the temperature where intergranular failure begins. According to nucleation theory, this decrease in strength is caused by a difference in fracture mechanism at high and low temperatures similar to that which requires the fracture strength of water to be one hundredth that of glass.

Engineers call the fracture of liquids under reduced pressure "cavitation." Cavitation seriously limits the design of ship propellers; in the presence of propeller surfaces and the surfaces of thousands of tiny marine plants and animals, the fracture pressure of sea water frequently is little less than the vapor pressure. Underwater sound generators cause cavitation where their vibrating surfaces are in contact with the liquid. Intergranular fracture of metals also could be called cavitation.

Prevention of the cavitation associated with engineering structures is difficult. However, there is a simple means for the prevention of cavitation damage in an interesting nonmechanical system. Cavitation occurs in flowers that are cut when the water in their stems is under tension. Water columns initially under tension snap back into the stalk when cut, pulling in air behind them. Trapped air will block water passages and will cause wilting and death of flowers even when they have been placed with their stems in water. It is possible, of course, to prevent this difficulty by cutting the stems a second time under water, removing the length of stem containing air blocks (Fig. 6). Few cavitation problems can be as easily resolved.

FIG. 6. Prevent cavitation in flowers by cutting stems under water.

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MINERAL SPIRITS AS SELECTIVE HERBICIDES IN CONIFER NURSERIES

A COMPARATIVELY new development in weed killers is the use of mineral spirits in forest nurseries as a selective herbicide. When applied as a fine, mistlike spray on seed beds of certain species of conifers they cause heavy weed mortality, with little or no damage to the tree seedlings.

These mineral spirits are of napthenic origin and usually have an aromatic hydrocarbon content of 10–20 percent by weight. The trade names of the petroleum products successfully used to date in conifer nurseries include Stoddard Solvent, Sovasol No. 5, Varsol, Stanisol, and Sohio weed killer.

This method has been used successfully in recent years to control weeds in carrots, parsnips, and in cranberry marshes. Heavier petroleum derivatives have given successful weed control in fields of guayule—a rubber-bearing shrub—and in citrus groves in California, but they have not given such favorable results in conifer seed beds.

When used in conifer nurseries, the mineral spirits are usually applied as a fine, mistlike spray using 25–100 gallons per acre (depending on species and age class) and pressures of 100 or more pounds. The application is generally made at full strength, although a few nurserymen do report using emulsions.

Most conifers tested to date appear to tolerate treatments with 50–80 gallons per acre without appreciable "oil burning" or mortality. Certain tree species, for example, white spruce just emerging from the soil in newly seeded beds, are subject to damage or mortality at higher levels of treatment; hence, conservative applications for four- to six-week-old white spruce appear to be in

the range of 25–35 gallons per acre. After this stage, white spruce will tolerate considerably heavier treatments. Larches are reported to be subject to heavy mortality, and spraying them with mineral spirits is not recommended.

The reaction of the mineral spirits on weeds and grasses is very rapid and striking. Within a few hours after application of the herbicide, the weeds begin to droop slightly. Most of them are dead and the foliage turns gray to tan in color after twenty-four hours. The percentage of weeds killed usually is in the range of 60–95 percent from dosages of 50–75 gallons per acre. Small weeds are easier to kill out than larger ones of the same species. Certain plants, mostly perennials, are difficult to kill out with the usual dosages; quack grass, clover, and Canada thistle are among these. Hence, a "mop-up" operation by hand weeding is often necessary after use of the mineral spirit sprays.

Even including the cost of supplemental hand weeding, the use of mineral spirits in forest nurseries has reduced weeding costs by 50–60 percent or more. Their value is greatest in first-year beds, when the small size of the trees precludes the use of mechanical cultivation for about six weeks after germination and when practically all weeding must be done by hand. The spray material usually costs from 20 to 25 cents per gallon, and costs of applying it are generally in the range of \$2.20–\$5.00 per acre if applied with power spray equipment. Costs per acre for spraying will usually amount to about \$20.00 per acre, with an additional outlay of \$4.00–\$12.00 per acre for supplemental hand weeding, bringing total costs to an average of about \$28.00 per acre. Weeding done exclusively by hand involves costs usually ranging from \$60.00 to \$100.00 per acre for a single weeding.

Experiments were conducted at the Hugo Sauer Nursery, Rhinelander, Wisconsin, in 1948 to work out some refinments in application technique with one- and two-year seedlings and three- to four-year-old transplants, indicating that the possibility of damage to buds and needles of trees may be reduced to a minimum by watering the beds by means of overhead irrigation immediately before application of the mineral spirit sprays. Spraying at night or in cooler weather, or growing the trees under shade, also reduced oil damage to the trees to an absolute minimum.

There is strong experimental evidence that the mineral spirits, especially in dosages of 75 or more gallons per acre applied in late spring or early summer, kill weed seeds which have soft, permeable seed coats and which are lying very near the soil surface. This clue offers the prospect of spraying beds several days before sowing the seed of the evergreens in late spring, thus keeping them fairly free of weeds in the critical one-month period after seeding. In the 1948 experiments at Rhinelander, the weed stand in beds of jack pine treated with 75 gallons per acre of mineral spirits had only 10 percent as many weeds as untreated beds thirty-two days after application of the sprays. All visible weeds had been pulled by hand just before the spray application.

The killing action of the mineral spirits is apparently quite different from that of the much-publicized 2,4-D products. Not much information of a fundamental nature appears to be available on the action of the former, and this phase of the work would seem to be an interesting field of investigation for botanists and plant physiologists.

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NEW OPTICAL GLASSES

Before 1880 the only types of optical glass available to lens designers were the flint-crown series, in which the addition of progressively increasing amounts of lead oxide led to a progressive increase in both refractive index and dispersive power, so that in effect there was a fixed relation between the dispersive power and refractive index of all available glasses (Fig. 1).

In order to achromatize any lens, the positive elements must be of lower dispersive power than the negative elements, and in those days this meant that the refractive index of the positive elements had to be low, and that of the negative elements, high. This had two adverse effects: On the one hand, it tended to make the Petzval sum large, giving a strongly inward-curving field; and, on the other hand, it made the surfaces of the positive elements strong and those of the negative elements relatively weak. As the lens on the whole is positive, this resulted in considerable amounts of zonal spherical aberration, and, also, indirectly in large residuals of all the other aberrations.

For two reasons, then, a crown glass was needed with low dispersion and high refractive index and also, if possible, a flint glass of high dispersion and low index. The invention of barium crown glass

in the 1880s by Abbe and Schott helped enormously to meet the first requirement, but the problem of the low-index flint was not solved at that time. (Incidentally, plastics and liquids mostly tend to have higher dispersion for their index than glasses, but these materials are generally undesirable for other reasons.)

With the introduction of barium crown glass, many new types of photographic lenses became possible, the first to be developed being cemented triplets of the Dagor type, in which the three glasses are common crown, light flint, and dense barium crown, in that order. Barium crown glass also greatly improved all lenses of the airspaced type (the Cooke Triplet and the Celor, for example) by weakening the surfaces of the crown elements and enabling the designer to shorten the lens without adversely affecting the Petzval sum. To be sure, some successful lenses of the meniscus type—e.g., the Omnar—were designed without using barium crown, but these were the exceptions, and they did not become very popular.

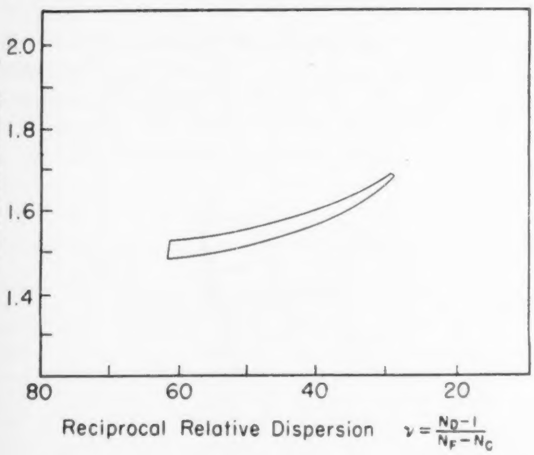

FIG. 1. Optical glasses (prior to 1880).

The refractive index of barium crown glasses was raised slowly and steadily by the Schott Company until the early 1930s (Fig. 2). At that time the company introduced SK-16 and SK-18, which marked the upper limit of the ordinary barium-soda-lime-silica types. These glasses are chemically unstable and present difficulties in lens manufacture because of their susceptibility to acid staining and their brittleness.

Following the first world war, C. W. Frederick, chief of the lens design department of Eastman Kodak Company, discussed the advances that were needed in optical glasses with G. W. Morey, who had done work of great value in the production of optical glass for military purposes. Frederick suggested that what was wanted was a very high refractive index with low dispersive power and that even a small production on a laboratory scale would be useful. Morey thereupon undertook to study the optical properties of glasses in relation to chemical composition, especially with a view to the production of high-index crown glasses. To this end all high-atomic-number cations were chosen for systematic study in silicate, borate, and phosphate glasses. Small melts of 20–40 grams were made to indicate the field of glass formation. The more promising ones were repeated in larger melts of 50–100 grams.

By 1933, the work had progressed to the point where silicon and phosphorus were discarded as glass-forming elements. Boric oxide had by now proved to be by far the best fluxing agent. Oxides of elements such as lanthanum and thorium, found in the rare earths, and columbium, tantalum, tungsten, titanium, zirconium, and strontium were used in major portions up to 80 percent by weight, with or without the usual barium, zinc, magnesium, and aluminum.

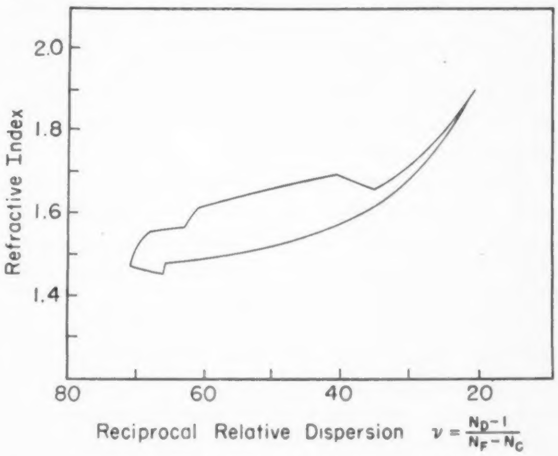

FIG. 2. Optical glasses (prior to 1934).

In 1934, samples of unusual glasses in the region with an n_D of about 1.85 and a ν of 43.0 were in existence and their properties well measured.[1] About this time the work was expanded to a larger scale by the Kodak Research Laboratories. Under the direction of S. E. Sheppard, a systematic study of the solubility of the rare elements in boric acid and of the limits of glass formation was made by L. W. Eberlin and P. F. DePaolis.[2]

The solubility of lanthanum in boric acid is remarkable, and its contribution to higher refractivity without increase of dispersion is a revelation. The oxides of tantalum, thorium, and tungsten are soluble in the lanthanum borate base glass

in amounts up to 35 percent. These new borate glasses are very stable and fairly hard. They are harder than flints, suitably stable to the atmosphere, and amenable to optical shop practices of molding, grinding, and polishing.

Early in the development work it was found that the new rare-element borate glasses were extremely corrosive to all known pot refractories. A decision was therefore made to use platinum for the actual production of these glasses. This was justified on the basis that no platinum would be lost by contamination and that the glass once homogenized could be poured in its entirety, free from striae, into a single slab or into cast shapes without striae, seed, bubbles, or other defects usually attending a glass made in a refractory pot.

The first of the new glasses to be made had a refractive index of 1.7445 and a ν-value (reciprocal dispersive power) of 45.8. The glass was slightly yellow, but further work established the origin of the yellow color, and finally glasses were produced as colorless and homogeneous as any other ordinary optical glass.

Pilot-plant operation began in September 1937, and the first commercial glass was delivered in June 1939. Production increased rapidly, and more than 125,000 pounds of rare-element glass were produced during the second world war (1942–45). Much of the success of the enterprise was due not only to the platinum equipment but to the Kodak method of using all-electric heating and a small-pot, "multi-pot-multi-stage" process.

Under sustained production conditions during the war, the yield of finished usable glass in a cast form was 95 percent of the theoretical glass available in the batch.

Electric heating was retained in a plant that was erected during the war and operated from December 1942 to September 1945 on a continuous basis of approximately 5,000 pounds of finished glass a month. The process consisted of feeding thirty-two 10-pound platinum-lined pots every twenty-four hours, starting one every three quarters of an hour and progressively moving these pots through the various stages of the glassmaking process.

Since 1940 the types of these glasses in production have been extended, and at the present time seven types are being made (Table 1).

All these glasses contain thorium, and in the case of folding cameras, in which the lens may rest for a long time in close proximity to film, the radioactivity of the thorium may be a disadvantage. A thorium-free equivalent of EK-320 is available.

The new glasses were first used in lens design in 1934, actual production of lenses began a few years later, and today many of the Eastman Kodak "Ektar" lenses contain the high-index glass. The versatility of these glasses is evidenced by the large number of lens patents that specify such glasses.

After the transfer of the production of the new glasses to the factory, the Kodak Research Laboratories continued their study of optical glasses. Theoretical considerations by M. L. Huggins and K. H. Sun[3] indicated the possibilities of further new glasses, and various new fields were studied. Of the glass systems of this type which were worked out by Sun, the most useful were flint glasses containing titanium oxide and using fluorine in addition to silica.

These unusual glasses lie well below the ordinary flint line (Fig. 3). For a given index, the dispersion is appreciably greater than that of ordinary flints. In this sense the glasses may be termed superflints. The best glasses lie in the region 45 percent SiO_2, 28 percent TiO_2, 27 percent NaF, ranging in optical properties from $n_D - 1.65/\nu - 29$ to $n_D - 1.58/\nu - 36.6$. The glasses are moldable, very resistant to tarnish, and easily fabricated by usual optical methods.

TABLE 1

	n_D	ν
EK-110	1.69680	56.2
-210	1.73400	51.2
-310	1.74500	46.4
-320	1.74450	45.8
-330	1.75510	47.2
-450	1.80370	41.8
-448	1.88040	41.1

The fluosilicate flints are almost as useful to the optical designer as the high-index glasses, since they extend the possible difference between crown and flint index and between crown and flint dispersion. With these glasses three-element lenses have been designed to give even better performance than the usual four-element types. Later, K. H. Sun succeeded in producing novel mixtures in some twenty quite different glass fields, including fluoborate, fluogermanate,[4] and fluophosphate systems.

A most interesting group of glasses suggested by Sun are those containing no oxides and composed entirely of fluorides. These glasses show the characteristic low refractive index and extremely low dispersion previously available only in fluoride minerals. The refractive index in most of the glasses approximates 1.38–1.39, and the ν-value,

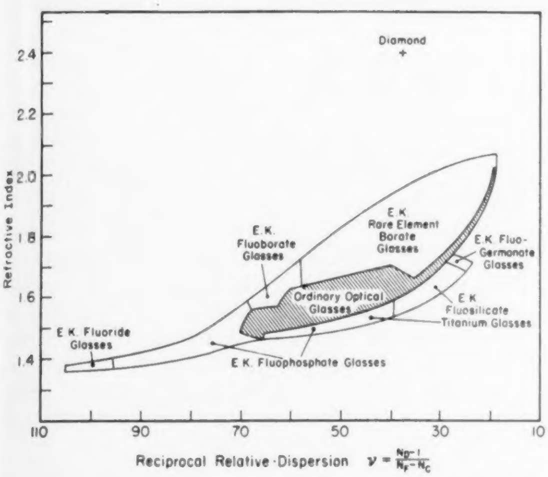

FIG. 3. Range of optical glasses before and after 1934.

100. Moreover, the glasses are transparent to below 300 mμ in the ultraviolet and to 5 μ in the infrared, so that they may be very useful in the making of instruments requiring optical transparency over a wide range of wave lengths. Difficulties have been met in the production of these all-fluoride glasses, but it is possible that these difficulties may be overcome in the near future.

The extension of the frontiers of optical glass by this work is illustrated in Figure 3. In this figure the range of optical glasses known before 1934 is shown crosshatched; the larger area shows the glasses that can be made at the present time.

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3. HUGGINS, M. L. and SUN, K. H. *Amer. Ceram. Soc.*, 1944, **27**, (1), 10–12, 13–17.
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ATOMIC CLOCK

Once time was reckoned only by the sun,
  Held in obedience to its golden strength,
By which a day was ended or begun:
  We gauged the morning by a shadow's length.

And then we gathered time into a glass,
  The moments falling with the sifting sand;
A man could all but see the seconds pass
  And grasp a minute in his naked hand . . .

Until we found that in a tiny spring
  Time coiled, and waited only to be free,
And so a pendulum began to swing
  To beat the tempo of eternity.

Yet still approximate . . . no accurate chime
  Before we tricked the atom's secret power;
But here at last is such exquisite time
  A million years won't swerve it by an hour.

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BOOK REVIEWS

SOCIAL HISTORY

The Family: Its Function and Destiny. Ruth Nanda Anshen, Ed. xi + 443 pp. $6.00. Harper. New York.

THE aim of this volume, to create a synthesis and understanding of the family, is conspicuously not achieved. The symposium is an aggregation of twenty quite independent papers, each of which may be worth while in its own right, but which contributes little or nothing to an integrated understanding of the family.

In the first chapter the editor pictures the American family as "moving with precipitous speed to greater atomization and destruction" but concludes by expressing the vague hope that "by manifesting a confidence and faith in absolute truth as it should be conveyed through the family in history, man may become willing to accept his historical responsibilities even though his historical strivings involve the deepest suffering and tragedy" (p. 17).

The book is divided into two parts: "Patterns" and "Structure." Family patterns are depicted as they are in Islam, China, India, Russia, and in Latin America, concluding with the Negro family and the American family. This last chapter on The Family: *Genus Americanum,* by the late Ruth Benedict, is a most significant contribution. She points out what everyone, except anthropologists and some sociologists, fails to perceive—namely, that the family in the United States is not in transition to disintegration but to a new form uniquely adapted to the American way of life. As compared with families in primitive, historical, and contemporary societies, American young people have freedom to select a mate by personal choice, a wide range of choices after marriage "about where to live, how the wife shall occupy herself, when to start a family, and a host of other important matters" (p. 112). Also, in this country the young couple have "an incomparable privacy," and "unusual potential leisure because of labor-saving devices, prepared foods, and ready-made clothes." Dr. Benedict emphasizes the correspondence of the distinctive non-authoritarian attitude of parents with the values stressed in our society. She concludes that "the family in the United States is an institution remarkably adapted to our treasured way of life. . . . Americans, in order to get the maximum happiness out of such a free institution as the family, need to parallel their privileges with an awakening responsibility" (pp. 168–69).

A miscellany of chapters is grouped under the heading "Structure": the social structure of the family, its emotional structure, social structure and anomy, the facts of life, education, housing, the crisis of the modern couple, the Oedipus complex, authoritarianism and the family, sex and character, religious values, and the family as conveyance of tradition.

The chapter by R. K. Merton on social structure and anomy is a brilliant analysis of differential reactions of Americans to our approved goal of success by conformity, innovation, ritualism, retreatism, or rebellion; but the relevance of his discussion to the family is only briefly stated. Denis de Rougemont asserts that the crisis of the modern couple arises from the failure of passion and romance as the sole basis for happiness in marriage and pleads for a new social realism, apparently unconscious that this trend is actually under way.

The concluding chapter by the editor signally fails to utilize the contributions to the symposium. She falls back instead upon a philosophical platitude: "The pursuit of truth, again established as the central purpose of society, will lead by its nature to the enjoyment of the good, the love of beauty, justice, and the reintegration of family life."

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MILITARY RESEARCH IN BRITAIN

Science at War. J. G. Crowther and R. Whiddington. 185 pp. Illus. 2*s.* 6*d.* H. M. Stationery Office. London. (American edition: iv + 185 pp. Illus. $6.00. Philosophical Library. New York.)

MOST scientists in university laboratories before the war were engaged in "academic" pursuits far removed from engineering applications. Yet these same scientists during the war turned, almost overnight, to engineering investigations and, permeating them with the spirit of modern science, were able to turn out veritable miracles of engineering production. In these war research laboratories there was a peculiar spirit of adventure, detective work, bold imagination, and daring, all tempered and guided by expert and logical mathematical reasoning. In this narrative, released last spring in Great Britain by the Department of Scientific and Industrial Research, through the Scientific Advisory Committee to the Cabinet, the authors try to recapture this spirit of the war research program.

The Advisory Committee selected as authors J. G. Crowther, chairman of the Association of British Science Writers, and R. Whiddington, F.R.S., head of the Department of Physics at Leeds University and one of England's best-known physicists.

The large amount of work and the diversity of subjects have made it necessary to concentrate on a few selected chapters. Almost half the total of 185

pages are devoted to radar, which was perhaps the most striking and effective contribution of British scientists to the war effort. Another 30 pages are devoted to operational research, a new technique introduced into warfare by the British. This is followed by a brief discussion of about 30 pages on the atomic bomb and, finally, by some 60 pages of selected topics under the title Science and the Sea. Britain, being a seafaring nation, had to solve a great many problems in this field which, to such an extent, did not exist for some of the other countries. Discussions such as Detecting Submarines by Various Methods, Sinking the Detected Submarine, The Magnetic Mine, and some of the others, are masterpieces of "case history" in the application of the scientific method, and every science teacher will profit from their study.

The chapter on the atomic bomb gives an excellent popular account of the science of radioactivity and nucleonics.

In the historical outlook on the atomic energy project, the authors, quite rightly, say:

> Through Rutherford and the British school of atomic physics, they made the major contribution in the sphere of physical knowledge of the atom. The British contributed the whole of the intellectual inheritance in nuclear physics to the pool. They contributed developmental work, but in Britain no plant for producing atomic bombs or for releasing atomic energy existed. Her physicists were dispersed and her engineers almost entirely engaged in other things.

Operational research was an entirely new feature, for the first time introduced into the techniques of war by the British. It was made possible because of the understanding for this necessity by such scientific leaders as Blackett, the famous cosmic ray physicist, and Mott, equally famous for his contribution of the theory of solid-state and quantum mechanics. It is interesting to read that this group of physicists, physiologists, mathematicians, etc. "apparently established its title of Anti-Aircraft Command Research Group by making a rubber stamp with the initials AACRG. It is not known whether any more formal authorization or recognition was ever obtained. The group became known as 'Blackett's Circus.'"

"The directors of operation research are generally civilian, and one reason why war in the future will tend more and more to be conducted in a civilian spirit. One reason why Hitler failed is that he was out of date" (p. 120). In its own press release, the Department of Scientific and Industrial Research says:

> The section on getting more out of aircraft illustrates the methods now being used to get more out of boilers and retorts. In the cotton industry, research has already been shown the way in which an overall increase in production of 20% can be obtained, and in the case of a few firms, can be doubled. Similar results are coming to hand in other industries.

The chapter on Radar is a classic example of how the scientific approach and the scientific method can be explained to the layman. A number of excellent drawings and diagrams, accompanied by a series of first-class photographs, both of equipment and of actual radar screens, and of the type of results achieved, are almost indispensable to anyone who wants to understand how it was possible, in an incredibly short time, to translate the knowledge of advanced electrodynamics into engineering practice and apply it to problems of extreme complexity.

> Major scientists such as Cockroft, Dee, Lewis, Oliphant and Skinner [all known to anyone who has ever worked in nuclear physics] helped to conceive military scientific problems of the most profound scientific point of view. . . . The great contribution of these men were the demonstrations of the fundamental importance of exact research. Rutherford was often criticized for training "Atom Smashers" who did not appear to be of any practical use to the nation. How right *he* was. His own nuclear physicists and their colleagues turned to radar, and in a few months helped to revolutionize it [p. 87]. As a comparison with the situation in Germany, we learned that in 1940 the German General Staff believed that they had won the war. They issued an order that no scientific research or development should be pursued which would not be of military use within four months. They drafted scientists into the armed forces to assist in the invasion of Britain. They did not realize that by the end of 1942 new scientific developments would be necessary if they were not to lose the war.

The authors have succeeded in bringing to us not only a picture of the war research and the spirit which is in the British Laboratory, but they also realize, to quote Sir Henry Dale's closing words in his preface:

> The use of science as an aid to war is a perversion from its proper purposes, and its rapidly extending misuse in the recent war, as a direct agent of violence and destruction on a stupendous scale, creates a threat to the survival of civilization. Meanwhile we may find some reassurance in recognizing that much of the discovery and invention which came so rapidly to hand, in response to the tremendous stimulus of the recent war's demands, will find immediate and beneficent use in peace.

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MEDICAL RESEARCH

Racial Variations in Immunity to Syphilis. Chester North Frazier and Li Hung-Chiung. xii + 122 pp. Illus. $2.50. Univ. of Chicago Press.

IS THE response to syphilitic infection manifested differently by Chinese, white, and Negro races? The senior author of this unusual little book reminds us that twenty-five years ago the belief was prevalent that the Chinese response to syphilis was different from the response of Europeans and Americans. This belief was not based on extensive studies of syphilis among Chinese. It was probably based on the old misconception that the manifestations of disease are bound to differ among different races since the mani-

festations of health also differ among them. With the fuller recognition that all peoples are basically the same biologically, the senior author well points out that differences in manifestations of disease among different races are more commonly due to environmental factors than to racial factors.

The book is unusual because the undertaking to collect data on Chinese patients in the Department of Dermatology and Syphilology at Peiping Union Medical College and on white and Negro patients in the Johns Hopkins syphilis clinic in Baltimore is unique. The authors were well qualified to carry out this extraordinary task, based on their extensive medical experience both in China and in the United States. It is particularly fortunate that they recorded their data dealing with the Chinese patients at Peiping. For the war has brought about marked environmental changes in China, and it will undoubtedly take many years before it will be possible again to undertake such careful medical studies among the Chinese.

The authors deal primarily with environmental and racial factors which influence immunity in syphilis. But their discussion reaches out beyond these factors and extends to those affecting health and disease in general. The book therefore should be of interest to both physicians and biologists. In addition, it is written in a popular vein and is exceedingly easy to read. This book makes an excellent companion to the classic little monograph on *Immunity in Syphilis*, by Alan Chesney, published in 1927. Additional monographs such as these two are needed for a more complete understanding of the multifaceted aspects of immunity in syphilis.

REUBEN L. KAHN

University Hospital
University of Michigan

ANIMALS, PLANTS, ROCKS, AND STARS

Fieldbook of Natural History. E. Laurence Palmer. x + 664 pp. 2,000 illustrations. $7.00, leatheroid binding; $5.00, cloth. Whittlesey House. New York.

THE encyclopedic format of this book gives a false notion of its contents. It "is not a textbook in botany, zoology, geology or astronomy . . . nor is it a manual for the identification of most of the objects considered in those sciences." Rather it is a stylized listing and illustration with concise comments on "the things that have interested" Professor Palmer, "his students, and his friends in more than a third of a century of teaching field natural history from New England to Hawaii." In addition to details about stars, rocks, and miscellaneous plants and animals, the *Fieldbook* includes information on domesticated and economically important plants and animals, about "cows, corn, cod and chickens," and various kinds to be found as ornamental vegetation and on display in zoological parks. Some of this material has appeared as special inserts in *Nature Magazine*.

Eighteen pages are devoted to astronomy, with specifications of magnitudes, periods, colors, distances, and positions of stars visible in the Northern Hemisphere, and with tabular data on the planets and their moons. Another 18 pages summarizes the mineral kingdom, with photographs heading triple-column pages, as in the rest of the book. One picture, one mineral, one column, is the uniform treatment accorded, mostly in agate (5½ point) type.

Over 300 pages survey the plant kingdom, with 940 kinds treated, each with a clear line drawing emphasizing general appearance, details of sori, buds, fruits, etc. Of these 33 are algae, 99 fungi, 15 bryophytes, 30 pteridophytes, 33 gymnosperms, 148 monocots, and 582 dicots. The selection is a curious assortment and, despite obvious efforts to the contrary, centers on the native and introduced flora of New England. Some may object to a column being given to the ornamental Mugho pine, whereas Western yellow pine, whose groves grace the West and whose wood builds modern houses, is accorded 4 ½ lines. Tropical plantain and breadfruit trees receive a column each, though users of the fieldbook are less likely to see them or to use their products. The colors of a number of flowers go unmentioned, including Nevada's state flower, the common sagebush.

Another 100 pages includes 955 kinds of animals, all but 27 of them mollusks (118), arthropods (198), or chordates. The photographs of clam and snail shells against a black background are less informative than the line drawings elsewhere. Amphibian egg characteristics, snake scale patterns, bird foot forms and egg details, mammal tracks and hair structure, are very helpful additions. Mammalian dental formulae, gestation periods, postembryonic schedules in many instances, dietary analyses, and in some cases body temperature, pulse, and respiration rates are cited where available. A full index completes the book, with common and generic names alphabetized, species under the appropriate genera.

Plant and animal classification is given inconspicuously in the text itself, to genus and species of each form discussed. Although supposedly exact, this practice may lead users of the book to assume that they can use it to identify species, even though no keys are provided and no claim to completeness is made. In several instances, as in the case of the carrion beetle and log-cabin caddis fly, the generic name follows the not uncommon misspelling. The horned toad illustrated and described (*cornutum*) is stated to be viviparous; actually it is oviparous, though other species of the genus do bring forth young alive. Here oviparity is less frequent than viviparity, but the specific name makes the statement wrong. Eight races of dogs receive a column apiece. Man is accorded a page of text and one of illustration, but no discussion of human races is included.

The book will have a useful place on the reference shelf, to be consulted in connection with reading, field trips, and visits to botanical and zoological

gardens. The pages reflect Professor Palmer's wide experience and his consistent interest in conservation.

Lorus J. and Margery J. Milne
Department of Zoology
University of New Hampshire

MAYA CULTURE BEFORE THE CONQUEST

The Maya Chontal Indians of Acalan-Tixchal: A Contribution to the History and Ethnography of the Yucatan Peninsula. France V. Scholes and Ralph L. Roys, with Eleanor B. Adams and Robert S. Chamberlain. v + 565 pp. $3.50 paper, $4.75 cloth. Carnegie Institution. Washington, D. C.

STUDENTS of Maya culture and of Middle America as a whole will welcome this scholarly and exciting study of the Chontal-speaking people of the pre-Hispanic province of Acalan, located in the southwestern porton of what is today the Mexican state of Campeche. The region of Acalan has been one of the least-known regions of the Maya culture area, both archaeologically and ethnographically. Prior to the present study there was little agreement even among specialists as to the precise location of this important province, not to mention the lack of knowledge concerning the people, their language and customs.

The book is based upon new documentary materials obtained by Drs. Scholes and Chamberlain in the *Archivo General de Indias*, Seville, Spain, during the thirties. The documents consist of the correspondence of colonial officials, missionary reports, law suits, administrative decrees, and other items. Perhaps the most interesting is the unique Chontal Indian text, the only one we have from the sixteenth century in this language. This text gives the history of the rulers of Acalan, going back six generations before the Conquest, lists the towns which comprised the province, and describes from the native point of view the arrival of the Spaniards under Cortes and the later Spanish activities in the area to 1602. From these documents the authors have reconstructed the aboriginal social, economic, political, and religious conditions in this area and have traced the effects of the Spanish Conquest through the seventeenth century.

The picture of Acalan before the Conquest, as it emerges from the documents, is that of an active and prosperous population of traders and merchants. Indeed, it is one of the important contributions of this study to show that the amount and extent of commercial activities in pre-Conquest times was much greater than in the later Colonial period. Trade relations extended across the Yucatan Peninsula from Tabasco on the west to the Caribbean and the Ulua River of Honduras to the east. Salt, cotton cloth, and slaves were exported from Yucatan in exchange for copper tools, cacao, feathers, gold, and other items. The authors conclude that the entire area from western Tabasco to the Ulua River was ". . . an economic block which, in spite of its political diversity, can be considered as a single commercial empire" (pp. 316–17).

The effects of the Spanish Conquest appear to have been disastrous. From 1530 to 1553 the Acalan population declined by about 60 percent, trade languished, and the people were reduced to subsistence agriculture in a poor land area. An especially severe blow to the native culture was the forcible resettlement of the natives to another area deemed more suitable by the Spanish priests. The authors conclude that disease and the disruption of the native economy were the two major factors in the population decrease. In this connection they make the interesting suggestion that malaria may have been introduced by the Spanish.

Oscar Lewis
Department of Sociology
University of Illinois

CIVILIZATION BY GEOLOGICAL CONSENT

Sedimentary Rocks. F. J. Pettijohn. xv + 526 pp. $7.50. Harper. New York.

EIGHTY-FIVE percent of the dry-land surface of the earth, and much of the sea bottom, are made of sedimentary rocks, of which sandstone, shale, and limestone are familiar types. A thorough knowledge of these rocks is an obvious necessity, not only because of their practical importance but because their bedded, or "layer-cake," arrangement gives us the framework for interpreting earth history. Yet there are only a handful of books that cover the nature and origin of these rocks in a comprehensive fashion. The present volume is an important addition to this small collection.

More than half of the book is given to a description of the mechanical and chemical composition of sedimentary rocks; this major portion includes eleven chapters. The first is a brief introduction. The second chapter is devoted to texture: size-distribution, shape, porosity, and other small-scale relations of the constituent particles. The third chapter covers chemical composition, and the fourth is given over to structure: larger features such as bedding, ripple mark, and stylolites. Two chapters are devoted to color and classification of sedimentary rocks, and then a chapter to each of the principal types: conglomerates and breccias, sandstones, shales and argillites, limestones and dolomites; and to the nonclastic sediments.

The remainder of the book includes a chapter on weathering, one on transportation of sediments, one on deposition, and a final chapter on lithification and diagenesis: how sediments become "hard" rocks and what may happen to them after deposition.

To this reviewer, Pettijohn's present contribution to the study of sedimentary rocks is twofold: First, he has summarized and brought up to date all the

pertinent material; his references include many foreign and unfamiliar but valuable works. In most cases he has re-expressed the contributions of other authors and with the aid of numerous diagrams and photomicrographs has given their work an added meaning. (A considerable part of this material, however, had been competently treated before.) Second, throughout the volume he has stressed the interrelationship of sediment, environment, and tectonics. In the chapter entitled Deposition he discusses this relationship in an engrossing and masterly fashion.

To whom will the book appeal? First of all, it is a book for geologists, primarily for those geologists whose interests lie directly in the field it covers. The amateur, or the scientist from another field, will find it heavy going. For the teacher and student of sedimentary rocks this volume will be a useful source book.

LINCOLN DRYDEN

Department of Geology
Bryn Mawr College

ENLARGED EDITION

Crucibles. The Story of Chemistry. Bernard Jaffe. xii + 480 pp. $3.95. Simon & Schuster. New York.

WHEN *Crucibles* was published in 1930 as The Francis Bacon Award (Book) for Humanizing Knowledge, Edwin Slosson described it as "the history of Chemistry told in biographies . . . with the necessary scientific explanation deftly worked in with as few repellent terms as possible."

That characterization may be accepted for this "enlarged edition" which, by minor revisions of two chapters and considerable editing and enlargement of the chapter on Langmuir, continues the story through nuclear fission. The three chapters added are entitled: Ernest O. Lawrence, Men Who Harnessed Nuclear Fission, and Nuclear Energy Tomorrow.

The author departs somewhat from the biographical pattern in the last two chapters. The reason for this in Chapter xviii, no doubt, is the difficulty of choosing any single individual whose contribution to the achievement of nuclear fission would give him preeminence in that accomplishment.

The reader is impressed by the new chapters as being vivid yet objective in their account of "a colossal task . . . completed in so short a time." The author pronounces "the goal of the ancient alchemists had not only been reached but had been left far behind." The alchemists had never dreamed there was an almost infinitude of "energy locked up in the heart of the atom."

The theme of the last chapter attempts to answer: What will this energy do to man, or can man do something worth while with it? It is in this closing section of the enlargement that the author lapses from the role of historian and prophet, at times, and becomes the propagandist. Even so, the reader closes the volume with the impression that, although the energy of fission has its terrors, it also has its benedictions if puny man can but make a workable choice of that aspect of its potentials.

Both the index and the section labeled "Sources" make adequate recognition of the enlargement. Those who read the first issue will surely wish, in this edition, to bring "the dramatic story of the atom up-to-date." The book well illustrates the art of making a good volume better.

B. CLIFFORD HENDRICKS

Department of Chemistry
University of Nebraska

THE ENGLISH VIRTUOSI

Scientists and Amateurs: A History of the Royal Society. Dorothy Stimson. xiii + 270 pp. Illus. $4.00. Schuman. New York.

IT IS said that in spite of the world-wide fame that he achieved, the greatest disappointment of H. G. Wells' life was that he was never invited to become a member of the Royal Society. Yet, what he considered to be the churlishness of the Royal Society in confining its membership to scientists who have achieved great distinction in their various fields, represents the main factor that enabled the Society to attain the famed position it now holds throughout the world.

For, in the early days of its existence, a majority of its members were gentlemen of the leisure classes, of no scientific attainments, well endowed with money, and interested in the advancement of learning for its own sake. Their financial support enabled the Society, almost always financially embarrassed, to remain in existence, but they could not by notable scientific achievement effectively reply to the biting satire of many of England's foremost writers of the day—amongst them Samuel Butler, Swift, Shadwell, and Steele. A contemporary of Steele, after reading the Society's account of a child born without a brain, remarked that had it lived it would have made an excellent publisher of the *Philosophical Transactions*. But these criticisms nevertheless had the effect of forcing the Society to put its house in order by avoiding the discussion of trivialities. (No doubt the Senator who recently was dismayed to find a U. S. government publication on the sex of watermelons would have evoked a sympathetic response at this time.)

Again, the Society was beset by political difficulties (not the least of which was the suspicion aroused by its early attempts at international collaboration), difficulties which unfortunately are still only too prevalent in the present day.

By the latter half of the nineteenth century, however, the Society had become firmly established in its own premises and the scientists were at last fully in control of the Society's policy. Financial recognition by the government came in 1850 with first grant of £1,000 which, however, did not alter the fact that the Society remained a private body. But this assistance,

supplemented by that of many private benefactors, enabled it to extend its support of fundamental scientific research by financing laboratories, such as the Mond at Cambridge, and establishing research professorships.

To conclude this short note on Dorothy Stimson's stimulating history of the Society, one may reflect on two odd facts she mentions: The first is that, although the constitutional ineligibility of women for membership was removed soon after the first world war, it was not until 1945, and even then with some dissension, that the first two women members were elected. The second shows that as long ago as just after the Society's formation in 1662 men were thinking of synthetic substitutes for natural materials. The "Ballad of Gresham Colledge," a verse associated with a Mr. Charles Howard, poses a question ". . . wheather since without Barke there may be taning, some cheaper way may not be tryed of makeing Leather without Hyde."

S. G. Rison

British Commonwealth Scientific Office
Washington, D. C.

AVIAN ACTIVITIES

Bird Display and Behaviour. Edward A. Armstrong. 431 pp. Illus. $5.50. Oxford Univ. Press. New York.

THIS is a revision and extension of an early work by the same author published by the University Press at Cambridge in 1942 under the title *Bird Display*. General arrangement in the two volumes is the same, though the 19 chapters and 381 pages of the first edition have been extended to 22 chapters and 431 pages in the present one. As the new volume has a decidedly larger type bed per page, the amount of material is evidently expanded by nearly one third. The author remarks in the preface that he has been able to correct a few errors and to profit "by the comments of critics and reviewers."

The work is a valuable contribution and a volume that will find a useful place on the bookshelf of students of animal behavior and of ornithologists, both professional and amateur. The pages are replete with interesting and well-selected examples of avian activities expressed clearly and tersely, with a minimum of the useless technicalities found in some sources. A simple scheme refers to the extended bibliography, where further details may be located in the original texts. Discussion is under common names, there being an appendix that gives the scientific names for those who wish them.

The discussions outline personal opinions of the author in a field where there may be disagreements, but they are on the whole conservative. The book may be definitely recommended as a reference work for thoughtful and pleasant reading.

Alexander Wetmore

Smithsonian Institution
Washington, D. C.

BRIEFLY REVIEWED

Science Outpost. Papers of the Sino-British Science Co-operation Office, 1942–1946. Joseph and Dorothy Needham, Eds. xi + 313 pp. Illus. 25 *s*. Pilot Press. London.

The struggle of China's science and technology to survive and develop in the face of the last war is described with insight in this volume. *Science Outpost* takes the form of a potpourri of reports, excerpts from letters and diaries, radio broadcasts, addresses, articles, and some remarkable poems. The editors call it a miscellany, and it is, but it carries the force of a thoroughly unified work because the contributors, of whom the most frequent and one of the most interesting is Professor Needham himself, have succeeded in this collection in catching the personal quality and spirit of an indomitable effort. The book is well illustrated with sixty photographs and contains in addition maps and diagrams of the organization of science in China, a useful list of scientific papers by Chinese scientists transmitted by the SBSCO for publication in the West, and an index of scientific personnel in China.

Howard S. Mason

National Institutes of Health
Bethesda, Maryland

Health Instruction Yearbook, 1948. Oliver E. Byrd, Ed. x + 320 pp. $3.50. Stanford Univ. Press. Stanford, California.

The 1948 *Yearbook* is the sixth annual compendium of current information briefed from 321 magazine articles published during the year in 100 different health publications, including *The American Journal of Public Health, The Journal of the American Medical Association,* and *The Congressional Record.*

The material is organized in an excellent manner, and it covers a field so large that the student will find readily accessible such varied and pertinent facts as mortality rates; need for 14,000 more psychiatrists; lack of well-trained persons for school health programs; current life expectancy; density of population; superiority of meat to legumes; how a person can combat fatigue; number of admissions to mental hospitals; time spent by boys reading comic books; new streptomycin treatment of TB; preparation for an approaching hurricane; name and qualifications of the new Surgeon General; dishwashing; juvenile delinquency; famine in India; current public health trends; etc.

The format of the book is enhanced by a long bibliography, a list of magazine sources abstracted, an author index, a subject index, tables, numbered paragraphs and sentences, and outlines. Each of the 20 chapters is preceded by an explanatory comment.

I believe that Dr. Byrd, who is professor of health education at the School of Education, Stanford University, has again succeeded in making the *Yearbook*

both a textbook and a reference work for nurses, physicians, students, laymen, and others interested in public health. It promulgates current experience, current research, and current opinion.

JULIAN M. SCHERR

The Medical Library of Bellevue Hospital
New York

Boy's Book of Snakes. Percy A. Morris. viii + 185 pp. Illus. $3.00. Roland Press. New York.

The book is dedicated "to boys and girls of the out-of-doors." Uncritically, it is an excellent book for the general information of youth on our most abused and misunderstood animals. It is well illustrated, interestingly written, and should be a real help to the young scientist. As a matter of opinion, it seems unfortunate that the te-hnical names are listed at the back of the book instead of under the illustrations and through the text, where they would give the beginner a good start on scientific nomenclature.

It is also unfortunate that Mr. Morris apparently failed to have his manuscript reviewed by a herpetologist, for the book contains too many trivial misstatements, which the reader will eventually discover if he continues the study of herpetology. Two examples: Sea snakes are said to come ashore to lay; actually some have their young born at sea. Illustration of a rattler's rattle labels the broken terminal segment the "button." Actually, the "button" is the first segment which is present on a newborn snake.

CHAPMAN GRANT

San Diego, California

Wild Animals of the World. Mary Baker and William Bridges. 272 pp. Illus. $4.95. Garden City Pub. Garden City, N. Y.

Mary Baker made the pictures, 252 of them, attractive and with a style of her own, an outstanding series of animal portraits.

Each is accompanied by a short, pithy, journalistic account of the animal figured, with a description and a mixture of jungle and zoo lore collected by William Bridges, the able curator of publications of the New York Zoological Society. Bridges, trained as a journalist, has lived among animals, in and out of the zoo, for a number of years, and acquired a sympathetic understanding of them which is shown in these lively paragraphs.

With 101 different kinds of pocket gopher known, and 70 kinds of guenon monkeys, the book cannot be a complete natural history, as there is room for only one pocket gopher and one guenon monkey. Arranged alphabetically from *aardvark* to *zebu*, the book might be considered a short encyclopedia of the principal mammals of the world. Informative and well-written, it is good reading, and the illustrations (many of them in color) will delight both children and adults.

W. M. MANN

National Zoological Park
Washington, D. C.

The Chemical Arts of Old China. Li Ch'iao-p'ing. viii + 215 pp. Illus. $5.00. Journal of Chemical Education. Easton, Pa.

The scientific and historical shortcomings of this pleasantly printed and delightfully illustrated book are mentioned by Tenney L. Davis in his foreword. This makes it the easier to enjoy these reports about a strange part of our world where the old methods and attitudes are still alive, where "even today in every Chinese industrial chemical plant a god of the respective industry is installed, and seasonal sacrifice is piously offered" (p. 4). Only the fantastic tales of alchemistical efforts belong entirely to the past. Chinese inventions opened up new fields of chemical manufacture in early times and then remained stationary for many centuries. The two-thousand-year old Chinese rig for drilling salt wells was recently cited as a model for the modern cable rig of today's oil fields (*The Lamp*, November 1948). Gunpowder, paper, and porcelain were made in the old original ways until quite recently. The old fermentation process to produce soybean sauce requires more than one year for completion. In this field, recent efforts at modernization have been made, particularly by Togano, who introduced the use of the pure culture of *Aspergillus orizae*. Where Professor Li's sympathies lie becomes clear from his remark: ". . . Togano's method still fails to yield a sauce which will match the delicate flavor of those produced by the older process" (p. 179).

Ceramics are described in the longest of the fifteen chapters; one might have liked to see an additional one on the textile industries. Metals, ink and dyes, and fermentations are treated quite extensively. Here, as in the shorter articles on, among other things, salt, gunpowder, oils, cosmetics, sugar, paper, and leather, old tales and practical procedures are given.

We are vitally interested in China, and we need to understand it. This book, besides having great aesthetic value, contributes to the knowledge of a culture in which time seems to play an entirely different role than in ours. The impact of modern industries on such a culture is a serious matter.

EDUARD FARBER

Washington, D C.

CORRESPONDENCE

DEMOCRATIC NOMINATION

(Dr. Taylor adds this supplement
to his article of last year.)

An article on "Three Methods of Voting," in THE SCIENTIFIC MONTHLY for October 1948, prompted a number of readers to remark that sound voting is impossible without sound nomination. Of course those readers are right. We therefore, in what follows, consider nomination.

Under ideal conditions, any of the usual methods of nomination may put up satisfactory candidates. Often, however, the conditions are so far from ideal that these methods put up candidates that fail to represent and interest the voters; consequently, these methods discourage many potential candidates and voters, both about particular elections and about democracy.

The usual methods include nominations by a committee, nominations from the floor, and nominations by written petition. Nominations by a committee may seem, or be, dictatorial. Nominations from the floor are often made by the most impulsive or specially interested few. As other members wish neither to seem critical of those nominations nor to cause "too many nominations," someone moves that the nominations be closed. The motion is seconded and carried. Written nominations, each endorsed by a given number of voters, may represent specially interested groups. Thus nominations by any of the usual methods, if the voters are not unusually alert, may leave the voters at the mercy of special groups.

Probably no method of nomination can be perfect; nevertheless, there is a simple way for members of an organization of not more than a few hundred members to put up representative candidates. The method is Proportional Representation.

By this method as applied to nomination, each voter receives a slip of paper with as many blank lines as there are offices to be filled, or 5 percent of the membership, whichever is larger, up to 6 lines. (An organization in which "every soldier is a general" would have to allow more lines.) The voter is asked to write the names of the persons he would like as candidates, his first choice on the first line, his second on the second, etc., so far as he wishes within the number of lines. To give him time to learn and think about possible candidates, he is allowed several days in which to return the slip.

Since the voter knows that the slips are to be counted according to Proportional Representation, he is free to vote for whomsoever he wishes. In other words, he does not fear lest he "throw away his vote," or "split the party," or nominate someone he does not want; he knows that his slip will help to nominate either his first choice, or one of his later choices, according to how many of the other voters agree with him.

Let us suppose that this method is used by an organization of more than a hundred members. This organization has the rule that the final slate must contain twice as many names as there are offices to be filled; or if the voters fail to propose that many names, the slate must contain all the names that the voters do propose. The organization needs nominations for candidates for 3 vacancies on the executive committee. The secretary gives each member one slip with 6 blank lines on it. One hundred members write in names and return their slips. These slips together list, as first choices, 16 different names.

This organization's rule requires that the final slate contain, for the 3 offices, 6 names (if the voters propose that many). To find out which 6 of the 16 names represent best the 100 voters as individuals, the tellers array the slips according to first choices, determine the quota necessary to nominate a candidate, and transfer otherwise superfluous or ineffective slips according to those slips' subsequent choices, all as prescribed by definite rules. (See references, previous article.) The result is a fairly representative slate of six nominees. Thus, if 30 of the 100 slips presented names of conservatives, 56, moderates, and 14, radicals, then, as the rules work out, 2 conservatives, 3 moderates, and 1 radical are nominated.

If the voters know all the possible nominees well, and agree beforehand that their slips shall count as final ballots, the tellers can determine the larger quota and count the ballots not for nomination but for election. In the example before us, according to the rules for Proportional Representation, 1 conservative and 2 moderates would be elected. (If only one office were to be filled the counting should follow the rules not for Proportional Representation but for Majority Preference, as outlined in "Three Methods of Voting.") Usually, however, the voters need to learn who are the actual nominees and to gather information about them before choosing between them. To this end, a formal statement of each nominee's qualifications, furnished either by the nominee or his advocate, or by some perhaps more trusted source, can make for intelligent voting.

Elections, not for all offices, but for a basic few—e.g., for the members of an executive committee who appoint other officers—encourage the voters to adopt suitable methods of nomination and election, to make good nominations, and to vote intelligently.

Intelligent nomination and voting should advance both democracy and science, since, properly understood, democracy and science are inherently allied.

WILLIAM S. TAYLOR

Department of Psychology
Smith College

ERRATUM

I am very much chagrined to discover that the formula for melanin in my article on "The Genetic Approach to Human Individuality" (SMO, March 1949) contains several errors. The carbon atoms at the left of each ring in the bottom row (p. 168) should not have a hydrogen atom attached to them. Also, in two instances the double bond attached to nitrogen should be a single bond.

LAURENCE H. SNYDER

University of Oklahoma

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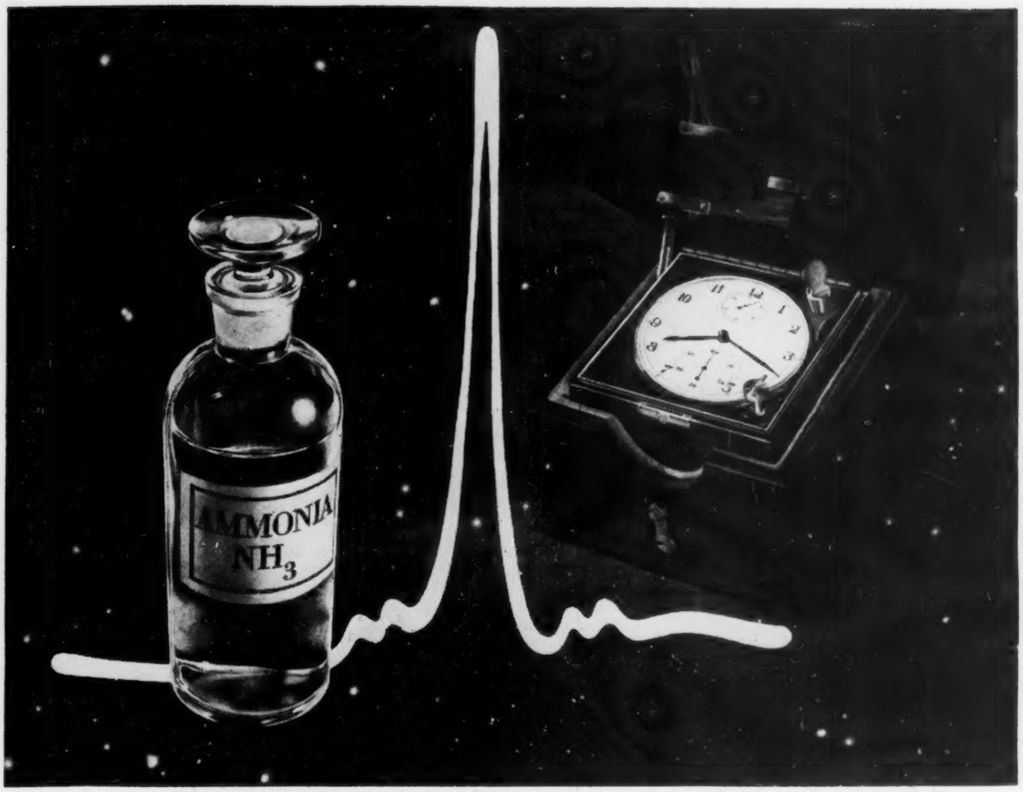

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Meetings

June 2. Manufacturing Chemists Association (Annual). Skytop, Pa.

June 5–10. American Society of X-Ray Technicians. Fairmont Hotel, San Francisco.

June 6–8. Division of Colloid Chemistry, ACS. University of Minnesota, Minneapolis.

June 10. Society of Motion Picture Engineers (Regional). Toledo, O.

June 10–11. East Tennessee Section, ACS. Oak Ridge, Tenn.

June 12–17. Special Libraries Association. Biltmore Hotel, Los Angeles.

June 13–14. National Association of Insecticide and Disinfectant Manufacturers (Midyear). Drake Hotel, Chicago.

June 13–15. National Fertilizer Association. Greenbrier Hotel, White Sulphur Springs, W. Va.

June 13–18. Pacific Division, AAAS, University of British Columbia, Vancouver, B. C.

June 15. American Society of Mammalogists (Annual). Washington. D. C.

June 15–16. American Meteorological Society (103rd National). Vancouver, B. C.

June 16–18. American Physical Society. George Washington University, Washington, D. C.

June 16–18. American Physical Society. Harvard University, Cambridge, Mass.

June 19–22. American Plant Food Council. Mt. Washington Hotel, Bretton Woods, N. H.

June 20–22. Division of Organic Chemistry, ACS. Madison, Wis.

June 20–24. American Society for Engineering Education (Annual). Rensselaer Polytechnic Institute. Troy, N. Y.

June 20–25. International Conference on "Science Abstracting" (UNESCO). Paris.

June 20–Sept. 2. Gordon Research Conferences, Colby Junior College, New London, N. H.

June 21–22. American Dairy Science Association (Annual). University of Minnesota, St. Paul.

June 22–24. Heat Transfer and Fluid Mechanics Institute (Annual). University of California, Berkeley.

June 23–25. American Society for X-Ray and Electron Diffraction. Cornell University. Ithaca, N. Y.

June 23–24. American Council of Commercial Laboratories (Annual). Curtis Hotel, Minneapolis.

June 24–25. Division of Analytical and Micro Chemistry, ACS. Wesleyan University, Middletown, Conn.

June 27–29. American Physical Society. University of Washington, Seattle.

June 27–July 1. American Society for Testing Materials (Annual). Chalfonte-Haddon Hall, Atlantic City, N. J.

July 13–15. American Society of Civil Engineers (Summer). Mexico City.

Aug. 15–19. International Dairy Congress, Stockholm.

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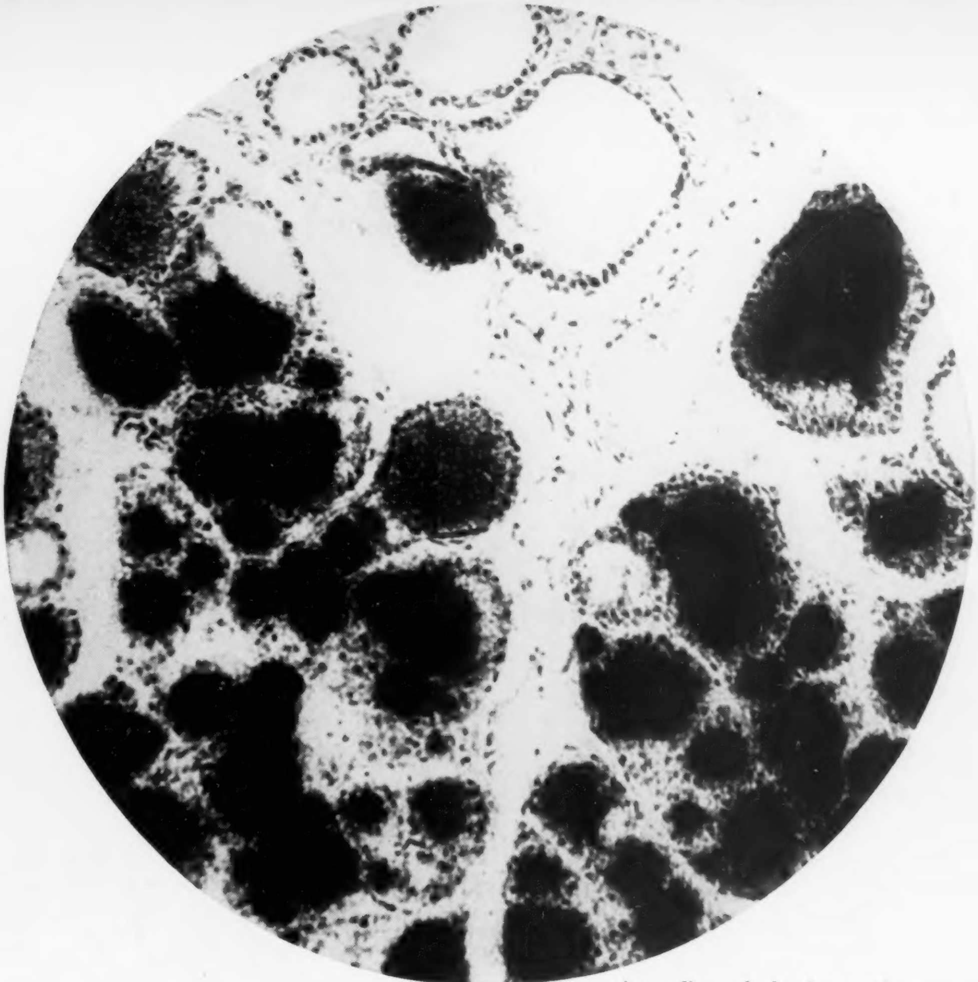

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